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PART III

THE DEVELOPMENT OF LIQUID- AND SOLID-PROPELLANT ROCKETS, 1880-1945

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COMPARATIVE ANALYSIS OF THE DESIGNS AND IMPLEMENTATION OF VEHICLES BASED ON REACTIVE PROPULSION PROPOSED DURING THE NINETEENTH AND BEGINNING OF THE TWENTIETH CENTURIES[+]

Victor N. Sokolsky (USSR)[++]

In studying the history of rocket technology, the question arises: to what extent were the ideas and proposals expressed during the nineteenth and beginning of the twentieth centuries actually realized? This report is devoted to a preliminary investigation of this question.

Examination of the presently known historical scientific literature related to the problem of reactive flight indicates that considerable attention had already been given to this problem in the nineteenth century—suffice it to say that about 30 designs for reaction flying vehicles were proposed during this period. Inventors were attracted by the apparent simplicity of the solution to the problem of flight with engines based on the reaction principle. However, the authors of a majority of the designs limited themselves only to a presentation of a diagram of the engine or an account of the principle of its operation, giving neither plans for its structural development nor precise calculations of the amount of energy required for accomplishing reaction flight. Such an approach was typical of the nineteenth century and is indicative of the extremely low level of theoretical development attending this problem. None of these authors considered the reaction flying vehicle as an object of variable mass, their choice of energy sources was extremely random, and the theory of the flight of reaction flying vehicles remained completely undeveloped.

[+] Presented at the Fifth History Symposium of the International Academy of Astronautics, Brussels, Belgium, September 1971.

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An analysis of the designs of reaction flying vehicles developed and proposed in the nineteenth century (Table 1) shows that they can be divided into three groups, depending on the method for producing lift (the horizontal motion for the vehicles of all three groups was accomplished due to the reaction of ejected particles of matter):

Group one—aerostatic principle—lighter-than-air reaction flying vehicles; the lift is produced with a gas lighter than air.

Group two—aerodynamic principle—heavier-than-air reaction flying vehicles; the lift is produced by the flow of air around the support surfaces (wings).

Group three—rocket-dynamics principle—heavier-than-air rocket flying vehicles; the lift is produced by the reaction of ejected particles of matter.

The principle difference between vehicles in the second and third groups is that the atmosphere is necessary as a supporting medium for the flight of vehicles in the second group, whereas the atmosphere is not only unnecessary, but even detrimental, for vehicles in the third group, i.e., it produces additional drag.

Flying vehicles of the first group—reaction aerostats—appeared unpromising and did not undergo further development. Refinement of vehicles of the second group led eventually to the creation of jet aviation, while the vehicles of the third group led to the creation of long-range rockets.

It is also of interest to divide the proposed designs into groups depending on the energy source. As proposed, such sources included: compressed air or other gas, water or alcohol vapor, and also combustible products which divided themselves into three subgroups corresponding to solid-propellant rocket engines (SPRE), liquid-propellant rocket engines (LPRE), and jet engines (JE). From the point of view of examining energy sources, the greatest interest attaches to the designs of S. S. Nezhdanovsky who, during the first half of the 1880s, proposed a design of a liquid-propellant rocket engine operation with double-component propellant.[1] Liquid hydrocarbons served as the fuel, and nitric acid or nitric oxides as the oxidizer.

The development of designs for reaction flying vehicles in individual countries did not always coincide with the general direction of development of the ideas of reactive flight in the nineteenth century. Thus, for example, designs of vehicles belonging to the first group (reaction aerostats) predominated mainly in France, Italy, and the USA, while designs of vehicles belonging to the second group (reaction aircraft) predominated in England and Germany. Together with vehicles belonging to the first two groups, much consideration (six designs) was given vehicles belonging to the third group in Russia. Work on such vehicles was also conducted in Spain.

Vehicles in group three are of the greatest interest for the history of astronautics, i.e., they did not require the atmosphere as a supporting medium and could

TABLE 1
PROPOSALS FOR THE CREATION OF JET PROPULSION FLYING VEHICLES SUGGESTED DURING THE NINETEENTH CENTURY

Propellant / Vehicle Group	Compressed Air or Other Gases	Water or Alcohol Vapor	Combustion Products		
			Single-Component Propellant	Liquid Fuel + Air Oxidizer	Liquid Fuel + Liquid Oxidizer
Lighter-than-air vehicles (reaction aerostats)	Tretesky /1849/ Sokovnin (1866) Nezhdanovsky <1882> Teyer (1884)	Tretesky /1849/	Italia (1831) Tretesky /1849/ MacLaret (1852) Petersen (1892) Beatty (1893)	Lebedev <1892>	
Heavier-than-air vehicles (jet aircraft)	Nezhdanovsky <1882>	Golightly (1841) Battler & Edwards (1867) Nezhdanovsky <1884> Ganswindt (1887)	Mafioti (1858) Evald /1886/	Teleshev (1867) Nezhdanovsky <1889>	
Heavier-than-air vehicles (rockets)	Nezhdanovsky <1889> Fedorov (1896)	Ganswindt (1887)	Arias /1872/ (1876) Nezhdanovsky <1880> Kibal'chin /1881/	Arias /1872/ (1876)	Nezhdanovsky <1882 - 1884>

Notes: 1) Dates of proposals contained in published works (books, articles, patents) are indicated in the table in parentheses ( ); dates of proposals contained in reports or demonstrated at exhibitions by / /; dates of proposals contained in unpublished manuscripts or authors' notebooks by < >.

2) The table does not include proposals of E. Zhir, V. Angius, Sh. Luvriet, S. M. Nemirovsky, V. D. Spitsin, and several other authors, i.e., in the materials at our disposal there are insufficient data about the operating principle and design of the vehicles they proposed.

be applied in principle for flight in the vacuum of space. However, in all of the designs presented above, the authors considered the application of the principle of reactive motion only for flight within the terrestrial atmosphere. Not one of them, including Arias, Kibal'chich, Nezhdanovsky, Ganswindt, and Fedorov, whose vehicles did not require the atmosphere as a supporting medium, raised the question about the possibility of applying these vehicles for interplanetary flight. This application was first proposed and scientifically justified by one of the greatest scientists of modern times, K. E. Tsiolkovsky, whose name is inseparably connected with the beginnings of rocket and space science and technology.

The end of the nineteenth and beginning of the twentieth centuries are characterized by increasing interest in the theory of interplanetary flight. Isolated works devoted to this problem appeared in a number of countries—mainly in Russia and Germany. The appearance of these works, whose authors first attempted to justify theoretically the possibility of flight in interplanetary space and proposed scientifically sound designs of spacecraft intended to solve this problem, indicates that the foundations of the theory of space flight began to be formed in just this period.

The urge of mankind to explore other worlds had previously arisen in earliest antiquity; however, for a very long time that urge was abstract and speculative in nature, and was embodied in the most fantastic flight proposals. This was caused in part by the repeatedly changing ideas about the structure of the universe, which went through a long and complex evolution—from the idea of geocentricity and the Earth as the only inhabitable celestial object, to the present physical picture of the universe. Only in the last century and a half, in connection with the development of science and technology, did technically more valid space flight designs begin to appear—in the forms of super long-range artillery, circular railways, a gigantic sling, etc. However, none of them could be realized in practice. Only at the end of the nineteenth century was the only realistic way for solving the problem found—using flying vehicles based on the reaction principle.

Tsiolkovsky became interested in the problem of interplanetary flight in the 1870's and 1880's, and in 1897[2] he derived the now widely known formula of rocket dynamics that bears his name and established the dependence between rocket flight velocity V_{max}, discharge velocity of the products of combustion V_1, the mass of the propellant M_2, and the mass of the rocket structure M_1:

$$V_{max} = V_1 \ln\left(1 + \frac{M_2}{M_1}\right)$$

Of course rockets were well known long before Tsiolkovsky. They were used for fireworks and for delivering signals, for illumination, and as military ordnance. Many scientists and inventors worked on improving rockets, but not one of them proposed using them as a means for accomplishing interplanetary flight. On the other hand, many inventors, even before Tsiolkovsky, thought about the problem of flight in space, but none of them proposed using rockets for this purpose.+ Tsiolkovsky served to unite these two technical directions, justified scientifically the possibility of the application of the reaction principle for space flight, and developed the fundamentals of the theory of rocket dynamics.

At the beginning of the twentieth century a number of scientists and inventors, independently of each other as a rule and often not even aware of analogous proposals made by other authors, became occupied with the problem of space flight. Besides Tsiolkovsky and Ganswindt who began work in this area at the end of the nineteenth century, this problem occupied R. Goddard (USA), R. Esnault-Pelterie (France), H. Oberth (Germany), G. von Pirquet, F. von Hoefft (Austria), and other investigators (Table 2).

Considering the proposals in the theory of space flight suggested during the end of the nineteenth and the first third of the twentieth century, the very wide range of proposed energy sources is noteworthy—from solid propellants (dynamite cartridges and smokeless powder) to electrical and nuclear energy and radiation pressure. Also very typical, when considering the possibility of space flight, the authors of this group of designs gave much more attention to problems of determining the required amount of energy and the theoretical calculations of rocket flight.

An analysis of the Tsiolkovsky formula indicates that the most effective method for increasing the velocity of rocket flight is an increase in the discharge velocity of the products of combustion. Thus, efforts of scientists during this period were directed to the selection of the highest caloric propellants having the greatest caloric value. Starting from just these considerations, in 1903 Tsiolkovsky proposed liquid hydrogen and oxygen as propellant components. He calculated theoretically the value of the ideal discharge velocity equaling 5700 m/sec. "I do not know of a single group of substances," he said in justifying his choice, "which would liberate such a

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+It should be noted that references to rockets used for flight to other celestial objects are encountered in several science-fiction works such as, for example, "L'histoire comique des etats de la lune," Cyrano de Bergerac (1647 - 1650), "A Journey to Venus," Ashil Heriot (1865), and "From the Earth to the Moon," Jules Verne (1874). However, the topic of all these literary works was not scientific technical designs, but rather focused on the fantasies of the novelists.

TABLE 2
DESIGNS FOR ACCOMPLISHING SPACE FLIGHT PROPOSED AT THE END OF THE 19TH AND BEGINNING OF THE 20TH CENTURIES

Energy Source		Achievement of Space Velocities			Launch From Orbital Station and Flight into Space
		Using Single-Stage Rockets	Using Multi-Stage Rockets	Launch From Flying Vehicles	
Solid propellant		Ganswindt /1893/ (1899) Oberth <1909>	Goddard (1919)	Ganswindt <1901>	
Liquid propellants	Hydrocarbons + oxygen	Tsiolkovsky (1914)	Oberth <1928> Tsiolkovsky (1929)		
	Hydrogen + oxygen	Tsiolkovsky (1903) Goddard <1907 - 1909> Oberth <1912> (1923) Kondratyuk 1917 - 1919 Tsander /1923/ (1924)	Oberth (1923) Tsiolkovsky (1926 - 1929) Hoefft (1928)	Tsiolkovsky (1911 - 1912) Oberth (1923) Hoefft (1928)	Tsiolkovsky (1918) Oberth (1923) Tsander /1924/ Hoefft (1928)
Metals (structural material)		Tsander <1909>	Kondratyuk <1920> (1929)	Tsander (1924)	
Nuclear energy		Tsiolkovsky <1903 - 1911> (1912) Goddard <1907> Esnault-Pelterie /1912/ (1913) Tsander <1925>	Bing (1911)		
Electrical energy					Goddard <1906> Tsiolkovsky <1911> (1912) Ulinsky <1915 - 1916> (1920) Kondratyuk <1917> Tsander /1926/ Glushko /1928 - 29/ Oberth (1929)
Radiation pressures					Kondratyuk <1917> Tsiolkovsky <1921> Tsander /1923/(1924)
Energy source not indicated			Goddard <1909>	Goddard <1907>	Pirquet (1928)

Note: Dates of proposals contained in published works are indicated in the table in parentheses ( ); dates of proposals contained in oral reports and materials presented before various organizations are indicated in / /; dates of proposals contained in unpublished manuscripts and notebooks are indicated in < >.

tremendous amount of energy per unit mass of products with their chemical combination."[3]

This same propellant was later considered by R. Goddard (1907-1909),[4] H. Oberth (1912-1923),[5] Yu. V. Kondratyuk (1917-1919),[6] F. A. Tsander (1923),[7] and other investigators. However, the energy requirements of the propellant often conflicted with operational requirements. The application of such components as liquid hydrogen and oxygen involved great operational difficulties. Moreover, during the first quarter of the twentieth century the production of liquid hydrogen in quantities sufficient for practical requirements did not exist. Thus, the authors of a number of designs discussed less caloric, but safer and more available propellants, replacing the liquid hydrogen with various hydrocarbons (such as, for example, alcohol, gasoline, kerosene, etc.).

Others soon learned that there are fuels having a greater caloric value than the combination of hydrogen and oxygen. In 1909 Tsander first arrived at the thought of the possibility of using the structural materials of the interplanetary craft as fuel.[8] Beginning in 1917, he began experiments in the ignition of molten metals, and soon obtained numerical values of the caloric value of magnesium oxide and other materials.[9] During 1920-1924, Kondratyuk also wrote about the possibility of using high-caloric metals as fuel.[10] However, even this path—the application of high-caloric metallic fuel—did not make it possible to solve the problem of space flight during this time. Calculations indicated that single-stage rockets operating with chemical propellant could not (with a feasible mass ratio of propellant-to-structure) even achieve orbital velocity. Thus, scientists and inventors continued to search for other forms of energy significantly exceeding the energy of chemical propellants.

Nuclear energy was one such form. In one of the unpublished manuscript versions of the work "Exploration of Space with Rocket Devices," Tsiolkovsky had already indicated the possibility of using the energy of the atom, which upon disintegration releases "particles moving with the velocity of light (or close to it), i.e., 6000 times more rapid than particles of water vapor."[11] The proposal for the use of energy of atomic decay for space flight is also found in the manuscripts of Goddard in 1907.[13] R. Esnault-Pelterie struck upon the possibility of using nuclear energy for space flight during this same year. He expressed this idea in a report given in November 1912, and published in 1913.[14]

Electrical energy is another promising form of energy for space flight, wherein the propellant is accelerated to very high discharge velocities in the rocket engine. Tsiolkovsky[15] and Goddard,[16] independently of each other, had already arrived at the idea of producing electro-rocket engines during the first decade of this century. Tsiolkovsky first published this idea in the journal Vestnik vozdukhoplavaniya (1912).[17] Goddard conducted experiments directed toward producing an ion rocket engine in 1916-1917.[18] In 1920, he obtained a patent for "A method of and means for producing electrified jets of gas."[19] During this same year, F. Ulinsky (Austria) publicly discussed his idea of producing an electron spacecraft,[20] an idea he had conceived during 1915-1916, as several authors have verified.[21] The problems of using electro-rocket engines for space flight later occupied R. Goddard (1916-1929), Yu. V. Kondratyuk (1917-1919), K. E. Tsiolkovsky (1921-1925), F. A. Tsander (1926), F. Ulinsky (1927), V. P. Glushko (1928-1929), and H. Oberth (1929).

During the first third of this century, however, proposals to use these forms of energy for solving the problem of space flight could be considered only as potential developments; i.e., the state of technology did not yet permit creating the proposed engines in a form suitable for use in flying vehicles. Moreover, these low-thrust engines could find practical application only after overcoming the gravitational pull of the Earth and the escape of the flying vehicle into a space orbit. Thus, the problem of finding a practical near-term method for achieving space velocities continued to haunt the investigators.

A second way to increase the velocity of rockets, also following directly from the Tsiolkovsky formula, involves altering the passive mass of the rocket during flight. Analysis of the fundamental equation of rocket dynamics led to the conclusion that, to increase the velocity of a rocket in flight, it is necessary to eliminate unnecessary structural elements as soon as possible with the depletion of propellants, while retaining only those parts which are necessary for the subsequent normal functioning of the rocket. Starting with this, in 1909 Goddard arrived at the idea of multi-stage rockets.[22] In 1911, A. Bing obtained a patent[23] for "A vehicle for investigating the upper layers of the atmosphere," that also contained this idea.[+] [24] In 1912, Tsander also perceived the suitability of ejecting individual exhausted structural elements.[25] This same proposal is contained in the works of Kondratyuk of 1917-1919.[26]

But the multi-stage rocket idea was expressed most completely in 1914 by Goddard, who obtained a United States patent for a two-stage rocket during this year.[27] In 1919, he proposed using a two-stage rocket for sending a projectile to the Moon.[28] The multi-stage rocket idea for space flight was later developed by Oberth, who considered it in detail in his work of 1923,[29] and by Tsiolkovsky, who developed in 1926-1935 the fundamentals of the mathematical theory of multi-stage rockets.[30]

The use of the principle of multi-stage rockets permitted solving the problem of achieving space velocities in the shortest possible time. In fact, experimental work had already begun in the 1930s in a number of countries; an altitute on the order of 400 km was achieved in the 1940s with a two-stage rocket, and in 1957, the first

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[+] In this connection, there is no patent of A. Bing in the Soviet Union. Information about his design is given on the basis of reference 24.

artificial satellite of the Earth was sent into orbit with the help of a two-stage rocket with five engines. Multi-stage rockets are now very widely used. However, the application of this principle did not completely solve the problem of eliminating the negative effect of the passive masses of the rocket. The unnecessary parts of the rocket were simply ejected, without bringing any benefit. Thus, other investigators began to consider the possibility of using these parts for increasing the active mass of a rocket (i.e., the mass of the propellant).

Tsander (1909),[31] and somewhat later Kondratyuk (1920),[32] gave an affirmative answer to this question, proposing the use of the structural parts of the rocket as additional fuel.[+] This proposal was published in the 1920s,[35] although it has not yet received practical realization. Scientists and engineers at present cannot give a firm answer to the question about the probability of its implementation in the future. But this proposal was examined rather thoroughly in the 1920s, and excited significant interest.

The third method for solving the problem of achieving space flight velocities attracted great attention among investigators, particularly at the initial stage of the creation of the theory of space flight. It involved firing the rocket not directly from the Earth, but from a high altitude launch base which could be, in the opinion of the inventors working on this problem, either a high mountain or a flying vehicle that would raise the rocket to a significant altitude. Such a proposal was first suggested at the beginning of the twentieth century by G. Ganswindt, who proposed raising his spacecraft as high as possible with helicopters, and only then starting the rocket engine.[36] Analogous proposals (but with the use of aircraft—aerostats) were also suggested during the first quarter of the twentieth century by Goddard (1907),[37] Tsiolkovsky (1911),[38] Oberth (1923),[39] and von Hoefft (1928).[40]

This approach attracted the attention of inventors because it would eliminate the necessity of overcoming the drag of the lower, denser layers of the atmosphere, and would therefore economize on the amount of energy required. However, its practical accomplishment presented significant difficulties, completely insurmountable at that time; indeed, it has not yet been practically employed.

[+] It should be noted that the notebooks of Goddard for 1908[33] and also his manuscript of 1913[34] contain mention of "rockets consisting almost entirely of propellant" or even "consisting completely of fuel." However, on the basis of material of Goddard at our disposal, it is not possible to respond unambiguously to the question whether the proposal in this case was the use of elements of the rocket structure as propellant or simply consideration of the theoretical case with negligibly small mass of the structure as compared to the mass of the propellant (or a hypothetical design case with a combustible shell), especially as there is no direct indication of the use of metallic fuel anywhere in the materials of Goddard known in the Soviet Union.

Using an artificial satellite of the Earth as an "intermediate station," (i.e., to launch a spacecraft from an artificial satellite) appeared more promising. This proposal is encountered in the works of a number of investigators. In 1928 von Pirquet considered the problem of using artificial satellites of the Earth as intermediate interplanetary stations in detail.[41] Kondratyuk expressed a very interesting idea using an artificial satellite of the Moon as an intermediate base for interplanetary flights.[42] The idea of using artificial satellites of the Earth as intermediate bases is very promising, and is an element in a number of present designs for reaching distant celestial objects. However, even today this proposal, though considered in numerous designs, has still not obtained practical realization (because of its complexity).

Some investigators also considered the Moon, the natural satellite of our planet, and other celestial objects as intermediate interplanetary stations.[43] As a rule, they started from the premise that elements would be found on them which could be used as propellant components. They also accounted for their significantly smaller mass (as compared to Earth), which made escape velocity possible with significantly smaller propellant consumption. However, none of the mentioned investigators considered the problems of obtaining and processing the required materials, preparation of the launch complex, and other operations related to the technical organization for preparing and launching spacecraft once on the Moon or another celestial object.

Scientists working on the solution of the problem of space flight during the first quarter of the twentieth century also suggested using radiation pressure for flight in interplanetary space.[44] They gave the completely correct relation between the periods of operation of engines of the various types. For flight to and from Earth, at times when it is necessary to overcome the gravitational attraction of the Earth and to impart significant accelerations to the spacecraft, they indicated that one should use the energy of chemical propellants; after escape into orbit and at a significant distance from the Earth, with flight in interplanetary space, one should use the energy of radiation pressure.

Consideration of the proposals made during the first quarter of the twentieth century on possible methods for achieving space velocities for space flight indicates a very wide range in the proposed form of energy, as well as the structural-component designs for spacecraft. During this time, the following questions were examined:

- production of liquid-propellant rocket engines;
- application of high-caloric metallic fuel;
- other forms of energy (nuclear and electro-reaction engines, solar radiation pressure);
- the structural material of the rocket itself as additional fuel;
  application of multicomponent and multi-stage rockets;

- intermediate interplanetary bases in the form of artificial satellites of the Earth and other celestial objects;
- application of wings for a gliding descent to Earth and other planets having atmospheres;
- the relative motion of celestial objects for acceleration or deceleration of spacecraft.

At the same time, other problems were also considered: the launch of spacecraft, determination of the optimum angle of launch, search for optimum trajectories and flight modes, spacecraft heating with passage through dense layers of the atmosphere, life support systems for the crew, return of the craft to Earth, analysis of flight trajectories of spacecraft, and a number of other scientific and technical questions related to the problem of space flight.

Thus, the works of the theoreticians of astronautics in the initial period of the development of rockets and space science and technology that ended in the 1920s provided the fundamental solution of the basic problems and established the principles of the theory of space flight before reliable rocket engines and rocket vehicles had been produced in any single country. At the same time, many investigators were separated from immediate practical problems and very often looked for solutions in such remote areas as using electro-rockets, nuclear engines, solar radiation pressure, and the like. During the development of the theoretical principles of space flight, many other problems were raised and solved separately from the main stream of the development of rocket technology, without which the practical solution of the problem of space flight would have been impossible.

At the end of the nineteenth and beginning of the twentieth centuries, to be sure, rockets were no longer used as military ordance. Nevertheless, attempts were repeatedly made to reactivate this weapon, particularly in the years immediately preceding the First World War. The contemporary successes in the area of aeronautics and aviation augered a large military role for an air force in a future war. In this connection, attempts were soon made to produce a new type of military rocket for arming flying vehicles. Work was also conducted on producing rockets intended for field battles.

Analysis of the experimental work in the area of rocket technology carried out at the beginning of the twentieth century indicates that designers and inventors working on rockets had to solve, in essence, the same basic problems that confronted them in the middle of the preceding century: to increase the range and improve the accuracy of rockets. However, the progress achieved in other areas of technology now permitted solving many of these problems. At the beginning of the twentieth century, seamless steel cases began to find application in rocket construction and improved measuring instrumentation began to be used. Methods for stabilizing rockets became more refined. The majority of projects at the beginning of the twentieth century eliminated guiding rods and replaced them with other forms of stabilization—by applying stabilizing surfaces or employing the gyroscope effect.

But the level of scientific knowledge in the area of rocket construction during this period remained as low as before. Most of those working on the creation of new forms of rockets were not acquainted with the theoretical works in the area of reactive motion, and in a number of cases held to naive, often erroneous ideas about the cause and nature of the reaction force. They did not even attempt to solve the theoretical problems related to the velocity and range of rocket flight and, as a rule, were completely uninterested in such concepts as the efficiency of the rocket engine itself, or the entire rocket as a whole.

A basic deficiency in almost all rocket designs at the beginning of the twentieth century involved the adherence, as before, to comparatively low caloric propellants, such as black powder, as the energy source. This hindered the progress of rocket technology and led to tactical, technical, and operating data that differed little in essence from the rockets produced in the middle of the nineteenth century (comparatively short range, significant scatter, premature rupture of the rocket case). Thus, the problem of producing military rockets comparable with rifled artillery had not been solved up to the First World War. Satisfactorily operating flare rockets also had not been produced. Further improvements in rockets called for replacing black powder with an improved, higher caloric rocket propellant. Replacing gun powder with a higher caloric rocket propellant--smokeless powder—had been discussed often at the end of the nineteenth and beginning of the twentieth centuries by B. T. Unge, R. H. Goddard, I. P. Grave, V. A. Artem'yev, and other investigators. The greatest successes in this direction were achieved by Goddard, who successfully carried out experiments with smokeless powder rockets with completely satisfactory results.[45]

Rockets remained practically unused as war material during the First World War (except for individual cases of applying incendiary rockets against enemy aircraft). Other forms of rockets (signal, flare) were also little used during the war years. Attempts by various inventors to improve rocket projectiles were unsuccessful. Only at the very end of the war did Goddard develop and produce samples of successful, albeit experimental, military rockets.[46]

At war's end, interest in rocket projectiles declined sharply and work on them in a majority of countries was discontinued. At the same time, interest in the possibility of using rockets for interplanetary flight increased significantly. Beginning in the 1920s, influenced by the works of Tsiolkovsky, Oberth, Goddard, and other investigators occupied with problems of space flight, an ever increasing number of people began to think about the possibility of penetrating space; groups, unions, and scientific societies, joining people interested in these problems, arose in various countries as more investigators became involved in the scientific problems related to the implementation of space flight. Nonetheless, a majority of those working in this area erred significantly in estimating the possible time for accomplishing space flight (these estimates varied from a few years to several centuries)—either guessing much too soon, or much too far in the future.

One of the most complex problems confronting investigators in the area of astronautical theory involved the possible sources for financing space flight projects. This problem inevitably confronted anyone who attempted to move from theoretical speculation to the practical implementation of his idea. It is striking that virtually no one devoted himself to a clear accounting of what expenditures of effort and means were actually required for a program to accomplish space flight. However, it soon became clear that such expenses could not be managed by individuals, or by entire organizations, unless these organizations were interested in solving practical problems and obtaining concrete results. The renewed interest of military circles in rocket armament intervened.

At the end of the 1920s and the beginning of the 1930s, military interest made a definite impression on the subsequent development of rocket research and, to a certain degree, determined a break in the development of rocket technology. It also became clear during this time that space flight could not be accomplished in the immediate future, i.e., neither the contemporary scientific and technical potentialities nor the required material means were sufficient for this endeavor. The discrepancy between the high level of theoretical development and the contemporary technology limited by practical potentialities was very typical (at the end of the 1920s and beginning of the 1930s). Thus, the beginning of the 1930s marked a rather sharp transition, away from research directed toward space flight, and toward investigations of a strictly applied nature, directed toward the solution of specific problems confronting rocket technology.

Now, the development of rocket technology included a large group of investigators who could be referred to as the second generation of pioneers, who carried the burden of work in creating the first rockets of a new type (liquid propellant). To overcome the skepticism and guarded attitude of the broad masses, they had first of all to show that rockets actually could achieve specified altitudes and flight ranges. The specialists working during these years in the area of rocket technology directed their efforts mainly toward creating liquid-propellant rocket engines and ballistic rockets.

In the development of ballistic rockets (Table 3), one can clearly distinguish two main directions: solid-propellant and liquid-propellant rockets (SPRE and LPRE, respectively). The first of these—solid propellant rockets—continued the many

TABLE 3
BALLISTIC AND WINGED ROCKETS TESTED IN FLIGHT UP TO THE BEGINNING OF THE 1940s

Type of Propellant		Ballistic Rockets			Winged Rockets		
		< 50 kg	50 - 500 kg	> 500 kg	< 50 kg	50 - 100 kg	> 100 kg
Solid Propellant		RS-82 (1932) RS-132 (1933) Damblanc (1935) ARS (1935-1939) M-8 (1939) M-13 (1939)	Damblanc (1938-39) R-604 (1940-41) R-521 (1940-41)		Tilling (1932) 48/I (1934-35) 48/II (1934-35)	217/I (1936) 217/II (1936)	803 (1940)
Liquid propellants	Gasoline (kerosene) + oxygen	Goddard (1926, 1928-1932) Repulsor (1931) ARS No. 2-4 (1933-1934)	Goddard "A" (1935) Goddard "LA" (1936) Goddard "LB" (1936-37) Goddard "LC" (1937-38) Goddard "P" (1939-41)				
	Ethyl alcohol + oxygen	GIRD-10(1933) GIRD-07 (1935) RBD-01/2 (1936) R-03, R-06/B (1936-1938) ANIR-5 (1939)	A-2 (1934) "Avlavnito" (1936)	A-3 (1938) A-5 (1939)		06/III (216) (1936-37)	
	Kerosene + nitric acid (oxides of N)		R-604 (1940) R-521 (1940)				212/603 (1939) 803 (1940)
Hybrid propellants		GIRD-09 (1933) GIRD-13 (1933)			06/I (1934) 06/II (1935-36)		
Ramjet engine (chemical propellant)		Rocket of Merkulov (1939)					

centuries of development of military and pyrotechnic black powder rockets. However, in place of this unpromising form of propellant, the rockets of the twentieth century began to feature smokeless powder on a completely different basis—the so-called double-base powder consisting of nitroglycerin and nitrocellulose.

Double-base powders have a number of advantages: they possess higher caloric energy, are significantly less dangerous to handle, and withstand much longer storage times and significant variations in temperature. It is true that many disadvantages characteristic of the old powder rockets remained—the practical impossibility of regulating the thrust, the necessity of placing all the propellant in the combustion chamber, the impossibility of reigniting the engine, and the lower energy content of the powder as compared with liquid propellant. However, solid-propellant rockets also had a number of advantages, in particular their constant readiness, that permitted them to compete successfully in a number of cases with liquid-propellant rockets.

The second major direction—liquid-propellant rockets—was more promising for accomplishing space flight from the energy point of view, i.e., they could use much more energetic propellants. Moreover, the use of liquid propellants permitted their gradual introduction into the combustion chamber, which simplified the solution of whole series of structural and technological problems.

In an overwhelming majority of the rockets flight-tested during the 1930s, liquid oxygen, considered by investigators as most effective from the energy point of view and most promising for future space flight, was used as the oxidizer. However, its application involved significant difficulties of an operational nature. Moreover, under the specific conditions (with the scales of rockets of that time), the application of oxygen did not give the expected energy gain compared to the less caloric, but denser, higher-boiling oxidizers. This also resulted in the tendency to use high-boiling oxidizers in a number of designs which were more convenient in operational respects. The most successful work on higher-boiling nitric acid LPREs was carried out during this period in the USSR, where several dozen such engines were produced during the first half of the 1930s.[47] The best of these—ORM-50, ORM-52 and ORM-65—gave thrusts from 150 to 300 kg, with a specific impulse up to 210-215 sec. The experience in work on these engines was also later used for producing aviation LPREs.

In addition, a great variety of structural solutions, component designs, and methods for stabilization and control were typical for the liquid-propellant rockets of this time, because the theory of rocket design was still very poorly developed, and this work was almost exclusively empirical.

The initial period of liquid-propellant rocket development, which encompassed about 20 years (1926-1945), can be divided into three stages. The basic problem confronting investigators during the first stage (the end of the 1920s and beginning of of the 1930s) was verification of the possibility in principle of producing liquid-propellant rockets. The main attention of investigators during these years was concentrated on producing an operational LPRE. After this problem was solved in principle by the efforts of scientists and designers of a number of countries (USA, Germany, USSR, etc.), and it was practically proven that a liquid-propellant engine could operate and produce a thrust sufficient for launching a rocket vehicle, a new problem confronted investigators: to provide for extended, safe, and reliable operation. This stage encompassed the mid-1930s. The engine remained the principal focus of attention of scientists and engineers working in the area of rocket technology during both the first and second stages.

During this second period the investigators occupied themselves with such problems as the selection of the most convenient propellants, the methods for their delivery to the combustion chamber, the organization of a stable burning process, provision for a sufficiently reliable cooling of the combustion chamber and nozzle, and other problems related to providing stable and reliable operation of the enginer.[48] However, in the mid-1930s, when it became clear that the problem of producing a liquid-propellant rocket engine was basically solved (although a whole series of theoretical, structural, and technological problems remained), a new problem confronted investigators: the necessity of producing control systems capable of providing stable flight of rocket vehicles on a specified trajectory.

This problem was not new theoretically, i.e., the pioneers of theoretical astronautics had not ignored the problem of rocket vehicle flight control. However, there was no practical experience in working in this area, as there had been essentially no sufficiently thorough scientific investigation of this problem. During the third stage of development, compassing the second half of the 1930s and the beginning of the 1940s, even this problem was successfully solved in principle and, with consideration of the experience accumulated in producing automatic aircraft flight control systems, controllable rockets—already rather sophisticated for their time—were produced.[49]

The significant increase in the scale characteristics of liquid-propellant rockets was also a distinguishing feature of this stage. While by the mid-1930s the thrust of LPRE tested in rockets did not exceed 30 kg, the maximum launch weight of of rockets was no more than 165kg, and their flight range did not exceed 4-6 km; but rocket engines with a thrust of 25-27 T and ballistic rockets with a launch weight exceeding 12 T and flight range of about 300 km were produced in the 1940s during the latter stage.

Along with ballistic rockets, winged rockets were also developed and used comparatively widely during the 1930s for carrying out various experiments and flight investigations. They acted as an intermediate link between ballistic rockets and rocket gliders. Winged rockets were studied during this period in Germany (Tilling, 1932), in the USSR (RNII, 1934-1939), and in other countries. Winged rockets were later used during World War II.

The problem of producing jet aircraft was also solved in principle during the 1930s. The first practical attempts to accomplish manned flight in heavier-than-air reaction powered vehicles occurred at the end of the 1920s, when the first reaction gliders and aircraft with SPRE were tested in Germany.[50] Inventors dwelt initially on solid-propellant engines (ordinary powder rockets) as the propulsion plants for reaction aircraft, largely because these were the only reaction engines checked in practice. Moreover, their simplicity, accessibility, and comparative adjustability of production played a great role.

However, engineers soon learned that solid-propellant aviation engines have a number of serious disadvantages which make it practically impossible to use them as the main propulsion plants of flying vehicles (they were used somewhat later as supplementary engines—launch boosters). These disadvantages include:

- impossibility of regulating thrust or repeated starting of the engine;
- comparatively low energy content of the solid propellants then known;
- the high relative weight of the propulsion plant.

Thus, attempts to apply solid-propellant engines were soon abandoned, and a new proposal was suggested at the very beginning of the 1930s—to use liquid-propellant rocket engines having a number of advantages compared to SPRE as the main propulsion plant of flying vehicles. At the end of the 1930s and beginning of the 1940s, this proposal was realized with successful flights of such experimental aircraft and gliders as the He-176 (Germany, 1939), RP-318-1 (USSR, 1940) Me-163B-1 (Germany, 1941), BI-1 (USSR, 1942), Northrop MX-324 (USA, 1944), etc. (Table 4).

The successful flight tests of aircraft with LPRE demonstrated the possibility of using this type of engine as a main propulsion plant. However, excessive fuel consumption made its application economically inefficient. Therefore, neither in this period nor later (even to the mid-1970s) did liquid propellant rocket engines become very widely applied in aviation as the main propulsion plants of aircraft flying within the Earth's atmosphere.

This particular subject has obvious interest, because even at the beginning of the 1930s, in selecting the best possible type of reaction engine for aircraft, a majority of inventors and designers dwelt on liquid-propellant rocket engines and not on jet engines that had a number of obvious advantages and, in the final analysis, later became the main type of aviation propulsion plants used for reaction aircraft. This question—why jet engines (JE) lagged in development—requires more detailed investigation. However, several preliminary conclusions can be expressed. An essential role

TABLE 4
JET PROPULSION AIRCRAFT AND GLIDERS TESTED IN FLIGHT FROM THE END OF THE 1920s TO THE FIRST HALF OF THE 1940s

Engine Type	Jet Aircraft			Jet Gliders (Flying Laboratories)
	With 1 Engine	With 2 Engines	With 3-4 Engines	
SPRE				Valier-Opel (1928) Opel-Hetry (1929) Valier-Espenlaub (1929) Cataneo (1931)
LPRE	Henkel He-176 (6-1939) Messerschmitt Me-163B (4-1941) Bolkhovitinov BI-1 (5-1942) Northrop MX-324 (7-1944) Junkers Ju-248 (9-1944)		Oka-II (1944)	He-112 (1937) RP-318 (1940)
TJE	Henkel He-178 (8-1939) Glouster 40/E28/29/ (6-1941) DeHaviland DH 100 (9-1943) Lockheed F-80 (T-33) (1-1944) Henkel He-162A-1 (12-1944)	Henkel He-280 (4-1941) Messerschmitt Me-262 (7-1942) Bell P-59A (10-1952) Glouster G-41 (3-1943) Arado A-23B-2 (6-1943)	Junkers Ju-286VI (8-1944)	Me-110 (1941) Gota Go-145 (1941) Wellington (4-1942) E28/29 (3-1943) Lancaster (6-1943) F-9/40 (11-1943) Junkers Ju-88 (10-1943)
MOTOR COMPRESSOR ENGINE	Caproni-Campini No. 1 (8-1940)	Messerschmitt Me-328B (6-1944)		
PULSED RJE	Fieseler Fi-103 (4-1944)			
JE				115 Bis (1-1940)

Note: Numerical dates in parentheses refer to the month and year.

evidently was played by the relative simplicity of LPRE as compared to turbojet engines (TJE), and because at the beginning of the 1930s there was practically no detailed JE design. Also important at this time, the problem of applying reaction engines in aviation was being studied principally by investigators whose main interests lay in the area of the development of rocket technology and the theory of space flight, which also determined their interest in LPRE. Moreover, this type of propulsion plant promised an interceptor aircraft with a rapid ascent rate, capable of flying at great altitudes, i.e., under conditions where the atmospheric oxygen would be insufficient to use as an oxidizer.

All of these factors evidently caused attention to focus on LPREs at the beginning of the 1930s. However, engineers soon realized that rocket engines (both LPRE and SPRE) could not satisfactorily solve the problem of producing serviceable reaction aircraft, and that engines using the oxygen of the surrounding air as the oxidizer were more efficient. Various countries successfully applied this type of engine at the beginning of the 1940s in reaction aircraft, and it later received very wide application. Thus, our analysis of the designs of reaction flying vehicles proposed and developed in the nineteenth and first half of the twentieth centuries indicates that two principal groups of flying vehicles were developed during this period: ballistic rockets based on the rocket-dynamic principle for producing lift, and reaction aircraft based on the aerodynamic principle. Sustained work in these two areas of interest predetermined further advances in aerospace technology in the mid-twentieth century.

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THE ALLEGED CONTRIBUTIONS OF PEDRO E. PAULET TO LIQUID-PROPELLANT ROCKETRY[+]

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Not infrequently, the name Pedro E. Paulet appears in chronologies, articles, and books dealing with the specific subject of liquid-propellant rockets and the larger areas of rocketry and astronautics. Paulet (Figure 1), a Peruvian, is recognized largely because of a letter he wrote from Rome in 1927 to a Lima newspaper. In that letter he claimed to have engaged in liquid-propellant-rocket experiments while a student in Paris three decades earlier. Paulet's letter subsequently came to the attention of a Russian rocket and space flight popularizer who quoted extracts in a book published in German in 1929. The purpose of this paper is to review the background and examine the available evidence concerning the only known claim to liquid propellant rocket engine experiments in the nineteenth century.

References to the alleged work of Paulet are fairly widespread. Albert Hausenstein, for example, wrote in 1940:[1] "We should not fail to mention Pedro Paulet who carried out, in 1895, experiments with a liquid propellant rocket characterized by a surprizing performance. During the period 1900-1918, no successful progress was made in the field of liquid propellant rockets based on Paulet's discoveries." After World War II, George P. Sutton described the Peruvian's work in these terms:[2]

> It is not yet certain when the first liquid propellant rocket motor was invented. The first practical working rocket motor is claimed by Pedro E. Paulet, a South American engineer from Peru (1895). He operated a conical motor, 10 centimeters in diameter, using nitrogen peroxide and gasoline as propellants and measuring thrust up to 90 kilograms. He apparently used spark ignition and intermittent propellant injection. The test device which he used contained elements of later test stands, such as a spring thrust-measuring device. He did not publish his work until twenty-five years later.

[+] Presented at the Third History Symposium of the International Academy of Astronautics, Mar del Plata, Argentina, October 1969.

[++] Professor and Head, Science and Technology Applications Dept., Research Institute and School of Graduate Programs and Research, The University of Alabama in Huntsville, Huntsville, Alabama. At present, Office of the Administrator, U.S. Energy Research and Development Administration, Washington, D.C.

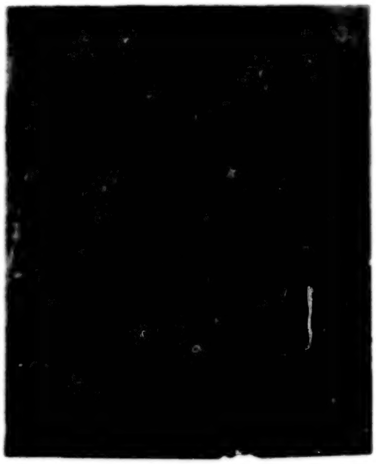

Fig. 1
Pedro E. Paulet c 1905

Sutton published in both editions of his Rocket Propulsion Elements a diagram (Figure 2) of the motor based on Paulet's written description. In the first (1949) edition, no direct reference is noted, but in the second edition he assigned Ref. No. 2.111: "P.E. Paulet, Liquid Propellant Rocket, El Commercio, Lima, Peru, 1920" [sic: 1927].

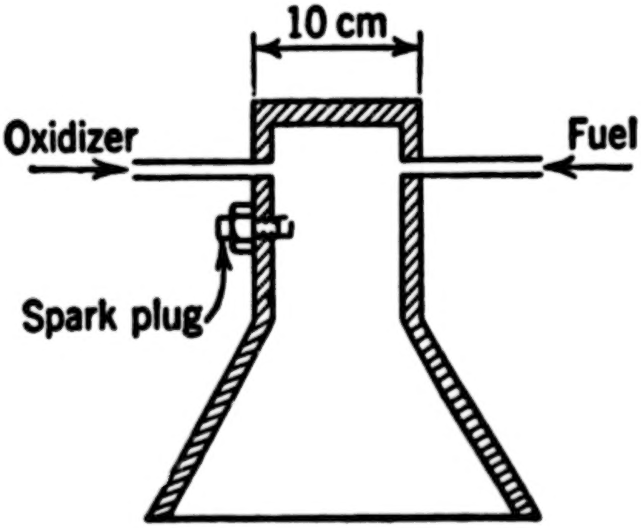

Fig. 2
George Sutton's Representation of Paulet
Thrust Chamber

In December 1946, a few years before Sutton's book appeared, the liquid-propellant rocket engineer, James H. Wyld, presented a survey paper, "The Liquid-Propellant Rocket Motor," to the Oil and Gas Power Division of the American Society of Mechanical Engineers in New York.[3] Wyld touched on Paulet's claim, and offered a schematic diagram not only of the thrust chamber but of the thrust-measuring spring dynamometer, timer, and propellant tanks (Figure 3). Sutton, who cited Wyld in his book apparently took no steps to check the authenticity of Paulet's claim.

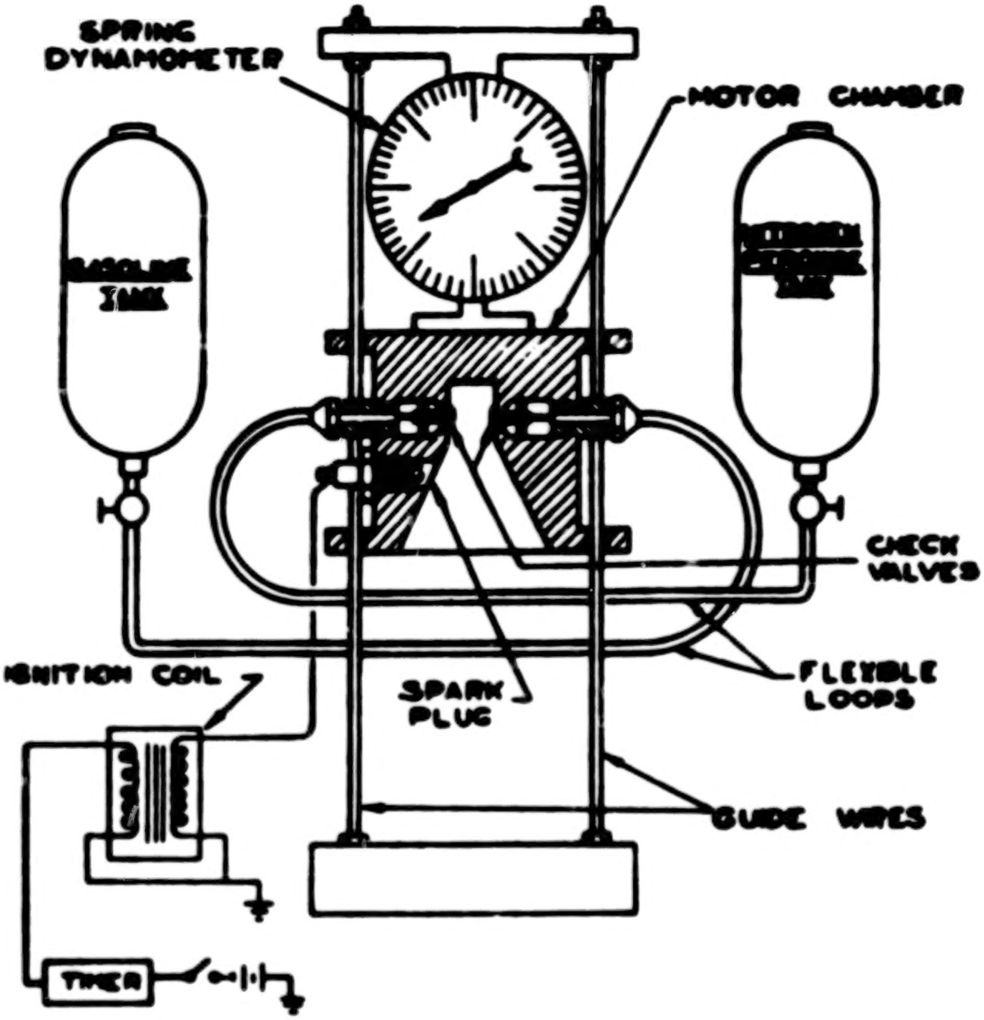
SPRING DYNAMOMETER
MOTOR CHAMBER
GASOLINE TANK
CHECK VALVES
IGNITION COIL
SPARK PLUG
FLEXIBLE LOOPS
GUIDE WIRES
TIMER

Fig. 3
James E. Wyld's Representation of Paulet's Liquid-Propellant Rocket

Nor did Wyld. But more cautiously, he asserted that the original source of the liquid-propellant rocket concept remained "quite uncertain," although "there is some evidence that the earliest practical working motor of this type was constructed in 1895 by Pedro E. Paulet, a young engineer of Peru, South America." Noting that Paulet did not publish an account of his work until 1927 in the Lima newspaper El Commercio (whose spelling, incidentally, is El Comercio), Wyld reiterated that "the validity of his claim may be rather doubtful."

Wyld, unlike Sutton, cited S.B. Scherschevsky's 1929 book Die Rakete für Fahrt un Flug as the source of information on Paulet's claim. Wyld simply translated from German into English Scherschevsky's own translation into German of extracts from the Spanish language letter in El Comercio. In a section entitled "First Practical Experiments," Scherschevsky stated categorically that liquid-propellant rocket motors were first tested by Paulet between 1895 and 1897. Notwithstanding the interval of three decades between the time of the alleged work and the time he got around to writing about it, Scherschevsky declared:

> At that time one could have experimented with rocket motors operating on liquid propellants. And one could have worked with imperfect propellants and materials. This must be said to the doubters and skeptics.

Doubters and skeptics there have been ever since. One of the first, Willy Ley, expressed strong reservations in his 1932 booklet, Grundriss einer Geschichte der Rakete.[5] He allotted Paulet's claim slightly over four lines, concluding: "The doubts are obviously correct." Ley never thought much of Scherschevsky, either. In the first edition of Rockets,[6] whose title was to expand over the years,[7] Ley complained:

> There are many legends connected with that early period which unfortunately acquired some permanence because a German-writing Russian by the name of Aleksander Borissovitch Sherschevsky (sic) uncritically put hearsay into some of his articles, and into his one and only book.

Scherschevsky, it appears, had gone to Germany to study gliders. But he overstayed his time and dared not return to his homeland. "Lazy by nature (and very proud of it), he earned just enough money for room and board by writing for professional journals," wrote Ley. "He could have done better if he had written about Russia but he didn't; he felt that what could be said at that time would not be considered too favorable by the Western mind, and he was genuinely in favor of the Soviet government. He was a refugee by accident." Between 1944 and his death 1969, Ley would not change his opinion about Scherschevsky, nor give further credence to Paulet's claims of experimenting with liquid-propellant rocket engines.

One who did believe in Paulet was the German, Max Valier. In 1930 he wrote that a liquid-propellant rocket motor of "astonishing power" had been developed by the Peruvian.[8] Valier seemed most pleased that "the 19th century did not close without a promising beginning to the technical development of rocket motors." He also felt that "the work of the Peruvian Paulet is most important for present projects leading to rocket ships, for it proved for the first time—in contrast to powder rockets burning only a few seconds—that by using liquid propellants, the construction of a rocket motor functioning for periods of hours would be feasible."

Paulet's claim has continued to arise intermittently. Following Ley's precedent, however, neither Pendray,[9] Ananoff[10] nor Williams and Epstein[11] credit the Peruvian in their books, although all refer to Scherschevsky in one context or another and certainly knew of his writing.[4] Ordway and Wakeford, in their International Missile and Spacecraft Guide,[12] simply state that Paulet "reportedly" tested a liquid-propellant rocket motor; Ordway and his coauthors ignored him completely in their two-volume survey of astronautics, as did Shirley Thomas in her eight-volume Men of Space,[14] and Eugene Emme in his History of Space Flight.[15] Bragg in Rocket Engines [16] and Glasstone in his NASA-sponsored Sourcebook of the Space Sciences [17] only mention the claim. Fritz, however, in Start in Die Dritte Dimension[18] accepts the claim at face value. Prior to Goddard, he wrote, the liquid-propellant rocket motor was tested only once:

> Between 1895 and 1897, Pedro E. Paulet, an engineer, constructed in Lima an apparatus whose combustion was not uniform, but rather consisted in a series of 300 explosions per minute. Pressure oscillated in accordance with this rhythm, which caused the motor's performance to diminish appreciably. Notwithstanding this difficulty, a thrust of 90 kilograms was produced.

Whether mentioned or ignored, no serious efforts were made to try to establish the validity of Paulet's claim, or even to search out primary documentation, until the mid 1960s. In 1966, von Braun and Ordway published some preliminary comments[19] after locating Paulet's 1927 article--which turned out to be a 2 1/2-column letter written in Rome and published in the October 7 issue of El Comercio (Figure 4). After drawing attention to the plans he was reading about in Europe for airplanes and spaceships, Paulet wrote that he, himself, had conceived of these ideas "THIRTY YEARS AGO [sic] when [I] was a student at the Institute of Applied Chemistry at the University of Paris." He also expressed the fear that his claims would not be believed, and called upon his former student friends in the Latin Quarter to tell the world of his experiments, which were, nevertheless, "made, truly, without witnesses."[20] In 1968, von Braun and Ordway published additional information on Paulet in their Histoire Mondiale de l'Astronautique.[22] They included a picture of Paulet and a schematic (Figure 5) he had allegedly drawn of a rocket-powered airplane, though it was not published until 1965,[23] long after his death. Another drawing, made in 1902 with captions in English, appears in Figures 6 and 7.

Correspondence and personal discussions with Peruvian scientific and cultural officials in Washington and London, with a member of Paulet's family in Peru, and with the editor of El Comercio, have since yielded further information about Pedro Eleodoro Paulet Mostajo's life and activities. He was born on July 4, 1875 in Arequipa, Peru. His father died three years later, and, under the care of his mother, he attended school in Arequipa (Figure 8) and later graduated from the local university. In 1895, the youthful Paulet left for Europe, entered the Institute of Applied Chemistry at the Sorbonne (University of Paris) in 1898, and received an industrial engineering degree in 1901.[+] The next year he

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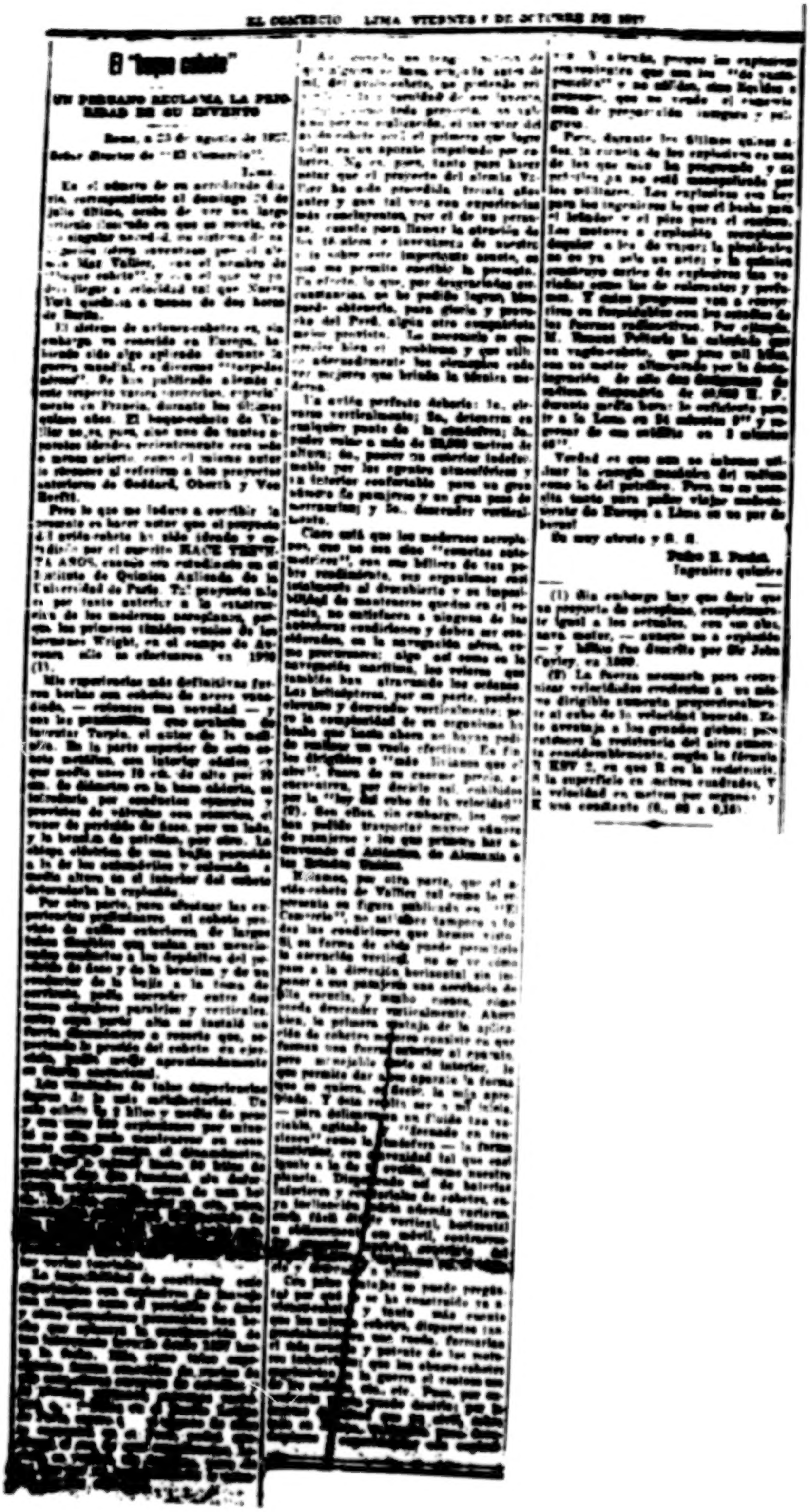
[+] If he did not start experimenting with rockets until after he entered the university, the 1895-1897 dates given earlier are incorrect. However, Paulet does mention, in other notes and letters, carrying out independent work beginning in 1895.



EL COMERCIO LIMA VIERNES 7 DE OCTUBRE DE 1927

El "buque cohete"

UN PERUANO RECLAMA LA PRIORIDAD DE SU INVENTO

Roma, a 23 de agosto de 1927.

Señor director de "El Comercio".

Lima.

Su muy atento y S. S.

Pedro E. Paulet.

Ingeniero químico

Fig. 4

Un ingeniero peruano es el precursor mundial de los aviones a retropropulsión

Pedro E. Paulet diseñó en 1902 el primer avión impulsado por cohetes

El ingeniero peruano Pedro E. Paulet que fuera fundador y Director de la Escuela Nacional de Artes y Oficios de Lima en 1904 y 1910, inventó en 1902 el avión sin hélices y está considerado como el precursor de la moderna navegación.

Desde niño, en su ciudad natal (Arequipa), antes de 1895 tuvo la idea de aplicar la propulsión por cohetes a la aerostación. Al viajar a Europa lo hizo con la idea de desarrollar su teoría, realizando en París diversas experiencias.

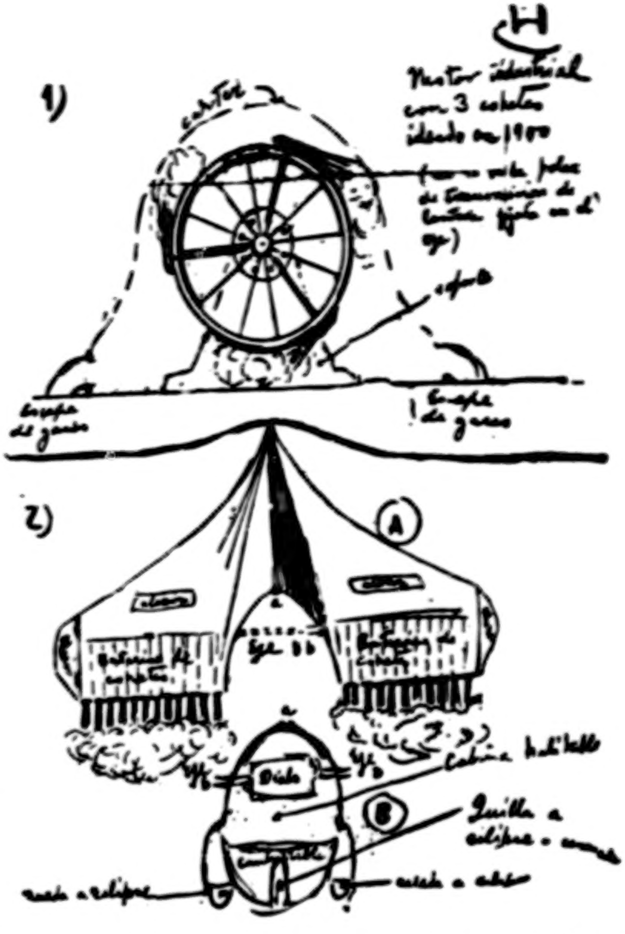
En 1902, en Bélgica, adoptó la forma de una nueva clase de aviones, que reproducimos en el grabado que acompaña a esta información y que nos ha sido proporcionada por el Presidente de la Comisión Encargada de Estudiar la creación de la Comisión Nacional del Espacio, doctor Manuel del Castillo, gracias a la gentileza de la familia del ingeniero Paulet.

El avión se componía esencialmente de un compartimento cerrado, en forma de huevo, de cuyo interior se manejaba una armazón externa que sostenía la proa del sistema, las dos alas y los timones posteriores. El compartimento era herméticamente cerrado y en su interior se podía viajar hasta la estratósfera o en la profundidad del mar.

Lo más original de su invento es el sistema de cohete que inventó y el explosivo utilizado en dicho cohete, que realizaba en ese entonces, la proeza de comprimir la mezcla explosiva y producir en seguida una explosión en campo abierto. En cuanto a la mezcla, utilizaba como base la gasolina o el agua.

El diario "El Comercio" publicó en el Suplemento Especial del 1º de enero de 1958 una información sobre el particular, destacando su labor de precursor mundial de la aviación, lo cual ha sido ampliamente reconocido en todo el mundo.

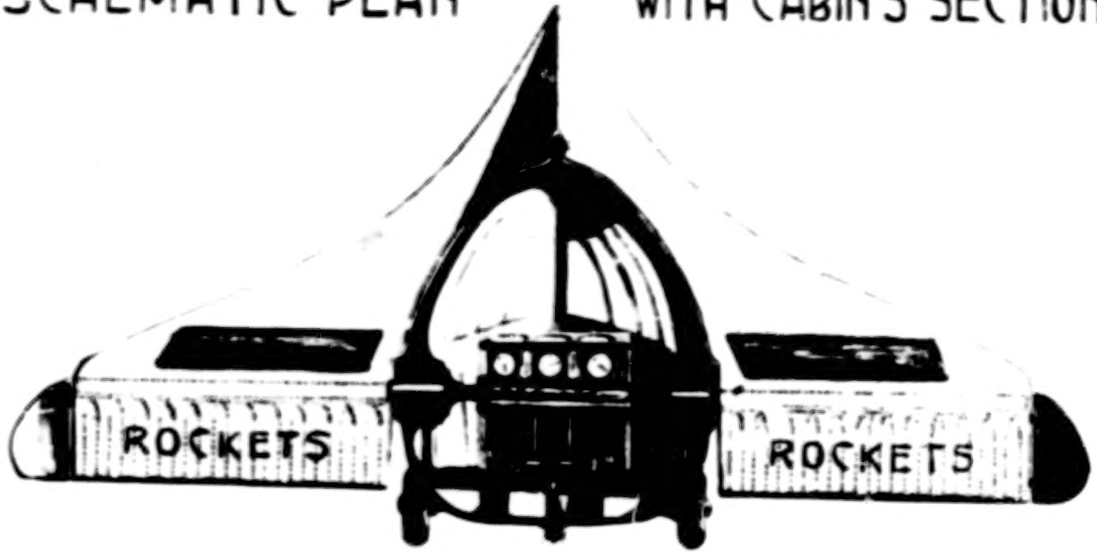
Como una primicia ofrecemos a nuestros lectores el grabado que acompaña a esta nota, en el cual, de puño del ingeniero Paulet, se aprecia el diseño de su invento.



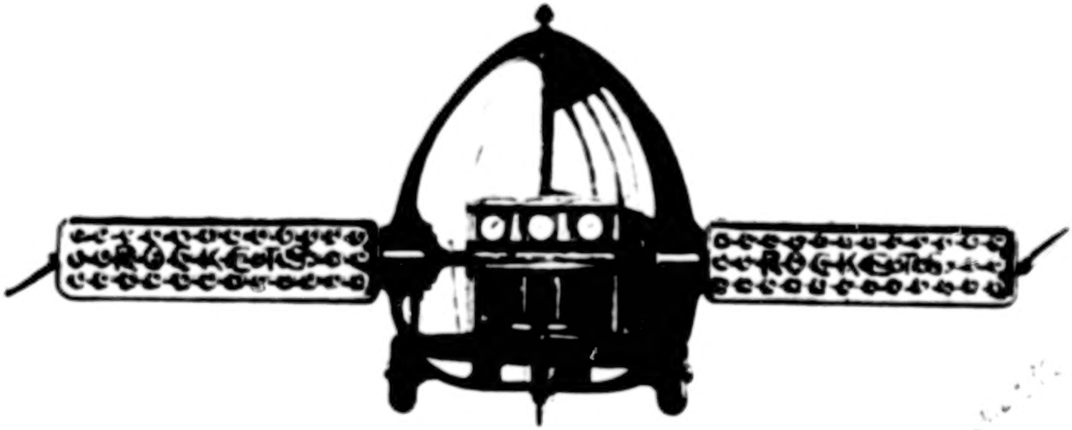
Esquema del primer avión-cohete inventado en el mundo, en 1902 por un peruano, el ingeniero Pedro E. Paulet. Por ello está considerado como el precursor de la navegación aérea a reacción, adelantándose a los inventores rusos y americanos

Fig. 5

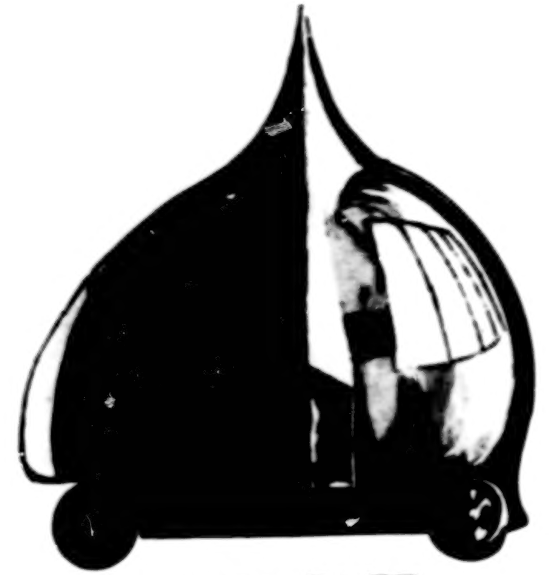
SCHEMATIC PLAN WITH CABIN'S SECTION


ROCKETS
ROCKETS

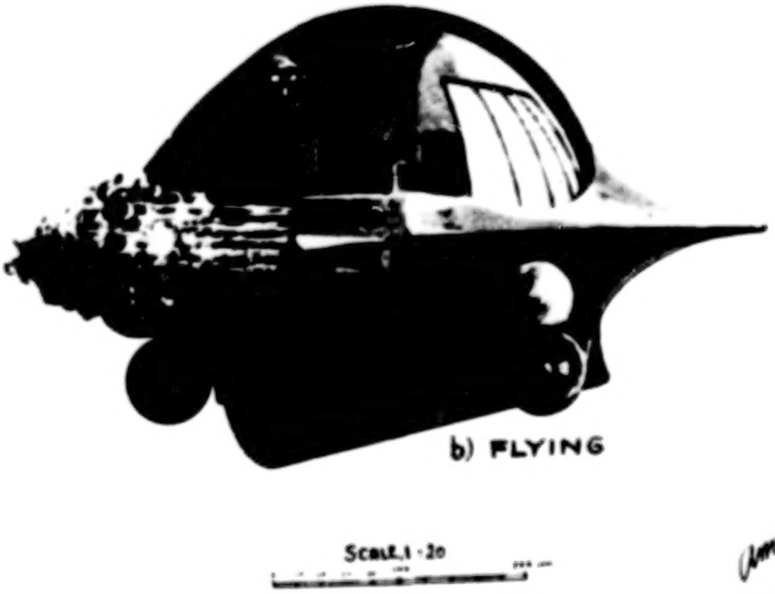
a) VERTICAL-FLIGHT


ROCKETS
ROCKETS

b) HORIZONTAL-FLIGHT



a) AT REST



b) FLYING

Amberes. 1 Mar. 1902

Pedro E. Paulet

Fig. 7
Avion-Torpedo System Paulet (1902)

On March [illegible], [illegible], possibly as a result of the inquiries in connection with the writing of references [illegible] and [illegible], El Comercio published an article, "A Peruvian Engineer Is the World Forerunner of Jet Propulsion Aircraft," that included the design of the "first rocket-airplane," already referred to. The article states that Paulet worked out the theory of rocket propulsion and conducted some early experiments in Paris, adopting the final design of his craft in [illegible]—when he had moved to Belgium. According to the

Fig. 8
Pedro Eleodoro Paulet Mostajo
b. Arequipa, Peru, 1874
Picture Made in Arequipa, 1883

description, the Paulet vehicle was to travel with equal ease out to the stratosphere or down into the depths of the sea. The batteries of rocket motors are clearly marked ("baterías de chohetes," Figure 5).

Next day, on March 11, El Comercio published[23] what it considered proof of the authenticity of the airplane, reproducing several artist's interpretations of the invention. The article first explained that many foreigners had written to El Comercio requesting details of Paulet's ideas and experiments, then summarized an interview given by the Peruvian in Buenos Aires in 1944 in which he had explained that his rocket airplane would fly at 600 miles per hour in the rarefied outer atmosphere; or, at much lower speeds, "travel like a submarine" under the ocean. He also related that:

> In my native city, Arequipa, built over lava from a neighboring volcano, we have no fear of fire. For this reason, rockets are the obliged entertainment in our celebrations. During my childhood, I learned how to make them, occasionally tieing to their "tails" nets with objects attached to them.

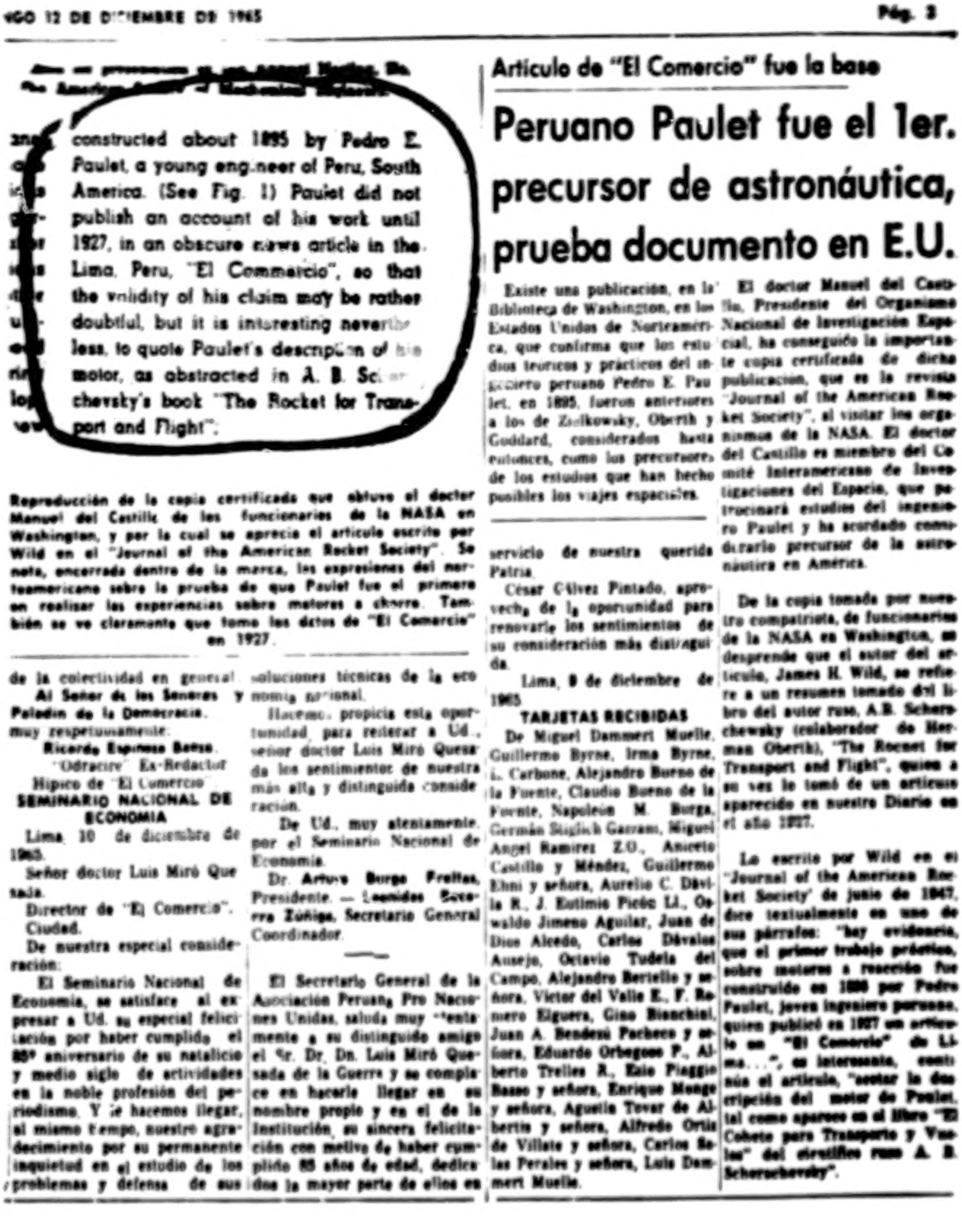
> Later, in Europe, while studying in the Institute of Applied Chemistry at the Sorbonne in Paris, I was seduced by the work of the great chemist Marcelin Berthelot on the power of explosive materials.
>
> Early in this century, there was great interest in the question of mechanical motors.
>
> At the Institute, we were taught that steam motors with performance less than 10 percent and which stood little chance of significant improvement, were impotent in the face of progress. The electric motor was not transportable except by generating power from heavy batteries. And the new internal combustion engine used in automobiles was relatively heavy and complicated for the new field of aviation, which was then such a novelty. It seemed to me then, as it does now, that the problem would be solved by utilizing explosive forces not in enclosed cylinders pushing a piston . . . but rather by rockets with constant injection of the explosive material and of simple design.

Unfortunately, neither of these references shed light on the supposed 1895 liquid-propellant rocket engine tests, the principal item of interest and the pedestal on which Paulet's fame rests. It is one thing to sketch a concept without supporting technical details and quite another to develop workable hardware. Efforts to obtain documentation on this from Paulet's son proved unsuccessful.

On December 12, 1965, another article[24] (Figure 9) in El Comercio stated that an American document proved Paulet to have been the world forerunner of astronautics. The evidence given to the newspaper by Dr. Manuel del Castillo, president of the Organismo Nacional de Investigacions Espaciales, consisted primarily of James H. Wyld's June 1947 article. But all Wyld had done, El Comercio had to admit, was to report what Scherschevsky had said. El Comercio did not quote or reproduce Paulet's 1927 letter from which Scherschevsky derived his information about Paulet.

It is perhaps ironical that the El Comercio article attached a "certified copy" of a short note appearing in an unidentified issue of the Journal of the American Rocket Society which says,"Paulet did not publish an account of his work until 1927, in an obscure news article in Lima, Peru, 'El Commercio' [sic], so that the validity of his claim may be rather doubtful, but it is interesting nevertheless to quote Paulet's description of his motor, as abstracted in A.B. Scherschevsky's book The Rocket for Transport Flight (see encircled upper left portion of Figure 7).

Living in Europe in the 1920s, Paulet certainly had an opportunity to become acquainted with the work and writings of German and French rocket and astronautical innovators. Just how many of his ideas were original and how many derived from these sources it is impossible to determine. Based on information available in October 1969, his claim of having experimented with liquid-propellant rocket motors in Paris in the late 1890s cannot be proved. To date, no actual witnesses have been located, nor any solid evidence uncovered as to the possible existence of the rocket motor.



GO 12 DE DICIEMBRE DE 1965 — Pág. 3

[illegible]

constructed about 1895 by Pedro E. Paulet, a young engineer of Peru, South America. (See Fig. 1) Paulet did not publish an account of his work until 1927, in an obscure news article in the Lima, Peru, "El Commercio", so that the validity of his claim may be rather doubtful, but it is interesting nevertheless, to quote Paulet's description of his motor, as abstracted in A. B. Scherschevsky's book "The Rocket for Transport and Flight":

Reproducción de la copia certificada que obtuvo el doctor Manuel del Castillo de los funcionarios de la NASA en Washington, y por la cual se aprecia el artículo escrito por Wild en el "Journal of the American Rocket Society". Se nota, encerrada dentro de la marca, las expresiones del norteamericano sobre la prueba de que Paulet fue el primero en realizar las experiencias sobre motores a chorro. También se ve claramente que tomo los datos de "El Comercio" en 1927.

Artículo de "El Comercio" fue la base

Peruano Paulet fue el 1er. precursor de astronáutica, prueba documento en E.U.

Existe una publicación, en la Biblioteca de Washington, en los Estados Unidos de Norteamérica, que confirma que los estudios teóricos y prácticos del ingeniero peruano Pedro E. Paulet, en 1895, fueron anteriores a los de Ziolkowsky, Oberth y Goddard, considerados hasta entonces, como los precursores de los estudios que han hecho posibles los viajes espaciales.

El doctor Manuel del Castillo, Presidente del Organismo Nacional de Investigación Espacial, ha conseguido la importante copia certificada de dicha publicación, que es la revista "Journal of the American Rocket Society", al visitar los organismos de la NASA. El doctor del Castillo es miembro del Comité Interamericano de Investigaciones del Espacio, que patrocinará estudios del ingeniero Paulet y ha acordado considerarlo precursor de la astronáutica en América.

De la copia tomada por nuestro compatriota, de funcionarios de la NASA en Washington, se desprende que el autor del artículo, James H. Wild, se refiere a un resumen tomado del libro del autor ruso, A.B. Scherschewsky (colaborador de Herman Oberth), "The Rocket for Transport and Flight", quien a su vez lo tomó de un artículo aparecido en nuestro Diario en el año 1927.

Lo escrito por Wild en el "Journal of the American Rocket Society' de junio de 1947, dice textualmente en uno de sus párrafos: "hay evidencia, que el primer trabajo práctico, sobre motores a reacción fue construido en 1895 por Pedro Paulet, joven ingeniero peruano, quien publicó en 1927 un artículo en "El Comercio" de Lima...", es interesante, continúa el artículo, "anotar la descripción del motor de Paulet, tal como aparece en el libro "El Cohete para Transporte y Vuelo" del científico ruso A. B. Scherschevsky".

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de la colectividad en general.

Al Señor de los Señores y Paladín de la Democracia,

muy respetuosamente.

Ricardo Espinosa Bossa.
"Odracire" Ex-Redactor
Hípico de "El Comercio".

SEMINARIO NACIONAL DE ECONOMIA

Lima, 10 de diciembre de 1965.

Señor doctor Luis Miró Quesada.
Director de "El Comercio".
Ciudad.

De nuestra especial consideración:

El Seminario Nacional de Economía, se satisface al expresar a Ud. su especial felicitación por haber cumplido el 85° aniversario de su natalicio y medio siglo de actividades en la noble profesión del periodismo. Y le hacemos llegar, al mismo tiempo, nuestro agradecimiento por su permanente inquietud en el estudio de los problemas y defensa de sus soluciones técnicas de la economía nacional.

Hacemos propicia esta oportunidad para reiterar a Ud., señor doctor Luis Miró Quesada los sentimientos de nuestra más alta y distinguida consideración.

De Ud., muy atentamente, por el Seminario Nacional de Economía.

Dr. **Arturo Burga Freitas**, Presidente. — **Leonidas Soverra Zúñiga**, Secretario General Coordinador.

—

El Secretario General de la Asociación Peruana Pro Naciones Unidas, saluda muy atentamente a su distinguido amigo el Sr. Dr. Dn. Luis Miró Quesada de la Guerra y se complace en hacerle llegar en su nombre propio y en el de la Institución, su sincera felicitación con motivo de haber cumplido 85 años de edad, dedicados la mayor parte de ellos en servicio de nuestra querida Patria.

César Gálvez Pintado, aprovecha de la oportunidad para renovarle los sentimientos de su consideración más distinguida.

Lima, 9 de diciembre de 1965.

TARJETAS RECIBIDAS

De Miguel Dammert Muelle, Guillermo Byrne, Irma Byrne, L. Carbone, Alejandro Bueno de la Fuente, Claudio Bueno de la Fuente, Napoleón M. Burga, Germán Stiglich Garrami, Miguel Angel Ramírez Z.U., Aniceto Castillo y Méndez, Guillermo Ehni y señora, Aurelio C. Dávila R., J. Eutimio Picón Ll., Oswaldo Jimeno Aguilar, Juan de Dios Alcedo, Carlos Dávalos Ausejo, Octavio Tudela del Campo, Alejandro Bertello y señora, Víctor del Valle E., F. Romero Elguera, Gino Bianchini, Juan A. Bendezú Pacheco y señora, Eduardo Orbegoso P., Alberto Trelles R., Ezio Piaggio Basso y señora, Enrique Monge y señora, Agustín Tovar de Albertis y señora, Alfredo Ortiz de Villate y señora, Carlos Salas Perales y señora, Luis Dammert Muelle.

Fig. 9

APPENDIX

Translation by the author of Paulet's letter written from Rome on 23 August 1927 and published in the Lima, Peru, newspaper El Comercio on 7 October 1927.

To the Director of "El Comercio"

In the issue of last 24 July of your esteemed newspaper, I have just seen a long, illustrated article which reveals an aerial navigation system invented by Max Vallier (sic), a German. He calls it the "rocket ship" and said that it would be able to reach a velocity such that New York would be less than two hours away from Berlin.

The rocket airplane system is already known in Europe, having been used in various "aerial torpedos" during the World War. A number of projects have been published in this regard—especially in France—during the last fifteen years. The rocket ship is, therefore, only one of many conceptions recently advanced—some accurate, some not so accurate. The author himself recognizes this by referring to the early pioneers Goddard, Oberth, and von Hoeftt.

I have been prompted to write the present letter to bring to your attention the fact that the rocket airplane project was conceived and studied by the undersigned THIRTY YEARS AGO when he was a student at the Institute of Applied Chemistry at the University of Paris. This project, therefore, antidates the construction of modern airplanes because the first timid flight of the Wright Brothers at the Auvours Field only took place in 1908.[1]

My most definitive experiments were conducted with rockets made of vanadium steel, then a novelty, and with panklastite that had just been invented by Turpin, discoverer of melinite. The interior of the metallic rocket was conical and measured 10 centimeters high by 10 centimeters at the open base (mouth). The propellants were introduced through the upper part of the chamber through opposing ducts provided with spring loaded valves. The nitrogen peroxide entered through one side and the benzine through the other. Ignition was effected by an electric spark from a spark plug, similar to those used in automobiles, which was placed halfway up the rocket chamber.

On the other hand, in order to undertake the preliminary experiments the rocket was provided with outside rings made of long flexible tubes which connected the above mentioned ducts to the nitrogen peroxide and benzine tanks and to a lead from the spark plug to the electric mains. The rocket would ascend between the two taut parallel and vertical wires, between whose upper part was installed a strong, spring thrust measuring device supporting the pressure of the firing rocket. The dynamometer could give an approximate measurement of the lifting forces.

The results of these experiments were very satisfactory. The 2-1/2 kilo rocket motor underwent 300 explosions each minute and was not only able to maintain its thrust against this thrust measuring device but was able to develop 90 kilograms of thrust. Moreover, it functioned without any noticeable deformations of its structure for nearly an hour. Under such conditions, it was possible to venture to foresee a craft provided with two batteries of 1,000 rockets each (one battery would rest while the other functioned) lifting several tons.

The impossibility of continuing these experiments with explosives, such as the nitrogen peroxide, whose handling was so risky, and other personal activities, have made it necessary for me to postpone my work on this interesting invention from 1897 to the present time. Moreover, because these experiments were known by several of my European student compatriots and by the few Peruvians (who at the time were very rare in Paris) living in the Latin Quarter, I hope that if one of them is now in Peru he will confirm the echo of these experiments which were made, truly, without witnesses but about which I talked to anyone who would listen to me.

Even though I do not have news of anyone who had concerned himself before me with rocket airplanes, I don't pretend to regain possession of my fathership of this invention because, as in all projects, it is not valid except as a consequence of its realization. The inventor of the rocket airplane will be the first one to fly in an apparatus powered by rockets. Therefore, it is not enough to say that the project of the German Vallier has been preceded thirty years earlier, and even perhaps by the more conclusive experiments performed by the Peruvian, who would like to call to the attention of the technicians and inventors of our country this important matter which has led me to write the present article. In effect that which, but for unhappy circumstances, I have not been able to achieve can be accomplished (for the advantage and good of Peru) by some other compatriot who is better prepared than I. It is necessary that he lay out carefully the problem and that he adequately use the elements offered by a continually improving modern technology.

A perfect airplane should: (1) rise up vertically; (2) maintain itself at any point in the atmosphere; (3) fly at an altitude of more than 20,000 meters; (4) possess an exterior which would not be deformed by atmospheric agents and whose interior would be suitable for a large number of passengers and a heavy load of merchandise; and (5) could descend vertically.

It is clear that modern airplanes are no more than "automotive comets" whose propellers give low performance, whose parts are almost completely uncovered, and which it is impossible to maintain motionless in space. These do not satisfy any of the earlier mentioned conditions and should be considered in aerial navigation as forerunners—somewhat as, in maritime navigation, sailing ships earlier crossed the oceans. Helicopters, for their part, can go up and down vertically but, because of their complexity, have up to now not been able to undertake effective flights. Finally, the dirigibles or "lighter than air craft," outside of their enormous cost, are also restricted because of the "law of the cube of the velocity."[2] They are nevertheless the craft that have been used to carry the greatest number of passengers and are the first to have crossed the Atlantic between Germany and the United States.

We noted, on the other hand, that Vallier's rocket airplane, like the one represented in the figure published in "El Comercio," satisfies neither of the conditions we have seen above. If its projectile form permits it to go straight up, one does not see how it changes to the horizontal direction without subjecting its passengers to acrobatic maneuvers. Even less do we see how it can go down vertically. Well, the first advantage of the application of rocket motors consists in that they form an outside force to the apparatus that is manageable from the inside, which permits it to have whatever form may be desired, i.e. that which is the most appropriate. This becomes in my judgement---in order to slide through a variable, stirred up, and "fecund in stress" fluid as the atmosphere---the lenticular form with convexity such that it is almost equal to that of an ovoid, such as our planet incorporating, thus, lower and horizontal batteries of rockets whose angle of firing could be varied. It would be possible to direct oneself vertically, longitudinally, and obliquely, resisting any contrary forces that the ambient fluid might produce, remain in space, and then descend to the ground.

With such advantages, one can ask why rocket airplanes have not been made, even more, why rockets have not been placed tangentially on a wheel which would form the most simple and most potent of industrial forces; and why rocket projectiles haven't eliminated the costly use of cannons in war, etc.,etc. Well, as a result of my own experience, I can reveal why: it is because of the great difficulty that a civilian has, especially in Europe, to obtain information on, and experiment with, explosives. Moreover, because the convenient explosives are of "juxtaposition" type and not solid, but rather liquid or gaseous, they are not sold commercially due to their uncertain and dangerous composition.

But, during the last fifteen years, the science of explosives has progressed from a practical point of view, and explosives are not now monopolized by the military. Explosives today are now to the engineers what the ax was to the woodcutter, the pick was to the miner. Internal combustion motors are replacing steam engines; pyrotechnics is no longer an art; and chemistry manufactures a series of explosives as varied as coloring compounds and perfumes. And this progress is going to advance into formidable studies of radioactive forces. For example, M. Esnaut (sic) Pelterie has calculated that a rocket ship, weighing 1,000 kilograms with a motor fed by the disintegration products of 2 decigrams of radium, would produce a force of 40,000 hp over a period of 1/2 hour, sufficient to be able to go to the Moon in 24 minutes, 9 seconds and return from this satellite in 3 minutes, 46 seconds.

It is true that even now we do not know how to utilize the mechanical energy of radium as we do that of petroleum. But, we don't need so much to travel modestly from Europe to Lima in a couple of hours.

Your humble and obedient servant,
Pedro E. Paulet
Chemical Engineer

(1) Nevertheless, it must be said that an airplane project, completely like those actually existing with its wings, body, and airship motors—even though it did not involve any explosions and propellers, was described by Sir John Cayley in 1809.

(2) The necessary force to transmit increasing velocities to a dirigible would proportionally increase as the cube of the velocity desired. This offers an advantage to the large globes; but, then air resistance goes up considerably, in accordance with the formula R KSV, where R is the resistance, S the surface in square meters, V the velocity in meters per second, and K a constant (0.08 to 0.16).

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MAIN LINES OF SCIENTIFIC AND TECHNICAL RESEARCH AT THE JET PROPULSION RESEARCH INSTITUTE (RNII), 1933-1942[+]

Yevgeny S. Shchetinkov (USSR)[++]

Over a period of one generation rocket technology developed from elemental experiments to a branch of industry that constitutes a rather large percentage in the state budget of all technically advanced countries. At first look it would seem that this rapid development of rocket technology started spontaneously just after World War II. However, considering this problem more attentively, it is obvious that the rapid development of rocketry in the USSR during the post-war years was due largely to pre-war activity; in particular, to investigations conducted in the Jet Propulsion Research Institute (RNII). For this reason the history of this institute is of special interest.

The history of RNII commenced on October 31, 1933, when the decree organizing the Jet Propulsion Research Institute was signed, creating in Moscow the first state-owned rocket research facility. RNII resulted from merging two rocket organizations: the Leningrad Gasodynamics Laboratory (GDL), and the Moscow Group for Study of Jet Propulsion (GIRD). Solid-propellant rockets were developed in the GDL from the beginning of its reorganization in 1927. Shortly thereafter, work began on jet-assisted take-off of aircraft and liquid propellant engines (generally with nitric acid as the oxidizer).[+++] In GIRD, projects of liquid-propellant rockets (generally with oxygen as the oxidizer), ram jet engines, rockets with and without wings, and rocket planes, were designed and investigated.

As the leading specialists of GDL and GIRD began to work in this new organization, these trends were continued in RNII. Thus, RNII was a collective of enthusiast-specialists in rocketry who believed in the great future of rocket technology, and, in

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[+] Presented at the Third History Symposium of the International Academy of Astronautics, Mar del Plata, Argentina, October 1969.

[++] Rocket engineer, affiliated with the Baranov Central Scientific Research Institute of Aviation Engine Construction, 1949; Chair of Combustion and Heat Transfer at the Moscow Physical-Technical Institute, 1960-.

[+++] See also, I. I. Kulagin, "Developments in Rocket Engineering Achieved by the Gas Dynamics Laboratory in Leningrad," and M. K. Tikhonravov, "From the History of Early Soviet Liquid-Propelled Rockets," in the Proceedings of the First and Second History Symposia of the International Academy of Astronautics, Smithsonian Annals of Flight, No. 10, 1974—Ed.

spite of a lack of funds, pursued intensive theoretical and practical studies. RNII was a real research and development organization that had at its disposal test facilities, scientific laboratories, work-shops, a flight test station, and launch complex. The scientific and technical board of the Institute met regularly. Scientific conferences were organized, and numerous scientific papers published. Over a period of 10 years, in fact, more than 120 papers were published in 19 editions and monographs. Many special reports (on the order of some hundreds) appeared, related to various concrete projects.

The projects completed in RNII were of different types. The purpose of some of them involved the practical application of rockets in the defense of the country. Many other projects dealt with the possibility of manned space flight. Various subjects of theoretical and applied sciences in rocketry were also studied (for example, problems of the gas dynamics, heat and mass transfer, etc.). The trends of RNII activity are considered briefly below.

SOLID-PROPELLANT ROCKETS AND THEIR APPLICATION IN ORDNANCE AND AVIATION

The fundamentals of chemical composition and production of homogeneous solid-propellant charges were developed in the GDL by V.A. Tikhomirov, B.S. Artemiev, B.S. Petropavlovsky, G.E. Languemac, and others. The technology was improved in RNII, and Yu. A. Pobedonostsev, L.E. Shvarz and other scientists achieved significant success in the design of charges and chambers.

Much attention centered on the problem of practical ordnance applications of solid-propellant rockets. A series of rocket missiles of 82 and 132 mm caliber were developed for launching from the ground or aircraft (Figure 1). The first successful use of these missiles from aircraft occurred in 1939 during combat operations at Khalkhim-Gol.[+]

To improve the aerodynamic characteristics of rocket missiles, engineers constructed the first Soviet supersonic wind tunnels in RNII under the direction of Yu.A. Pobedonostsev and M.S. Kisenko. Among them, the ejection-type wind tunnel of 400 mm diameter should be noted. Figure 2 shows a missile model in one of the first wind tunnels constructed in 1934-35.

Numerous studies were also carried out to improve rocket missile accuracy, which still left much to be desired. Significant progress was achieved in this field, and

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[+] See Yuri A. Pobedonostsev, "On the History of the Development of Solid-Propellant Rockets in the Soviet Union," in this Volume--Ed.

FIG. 1

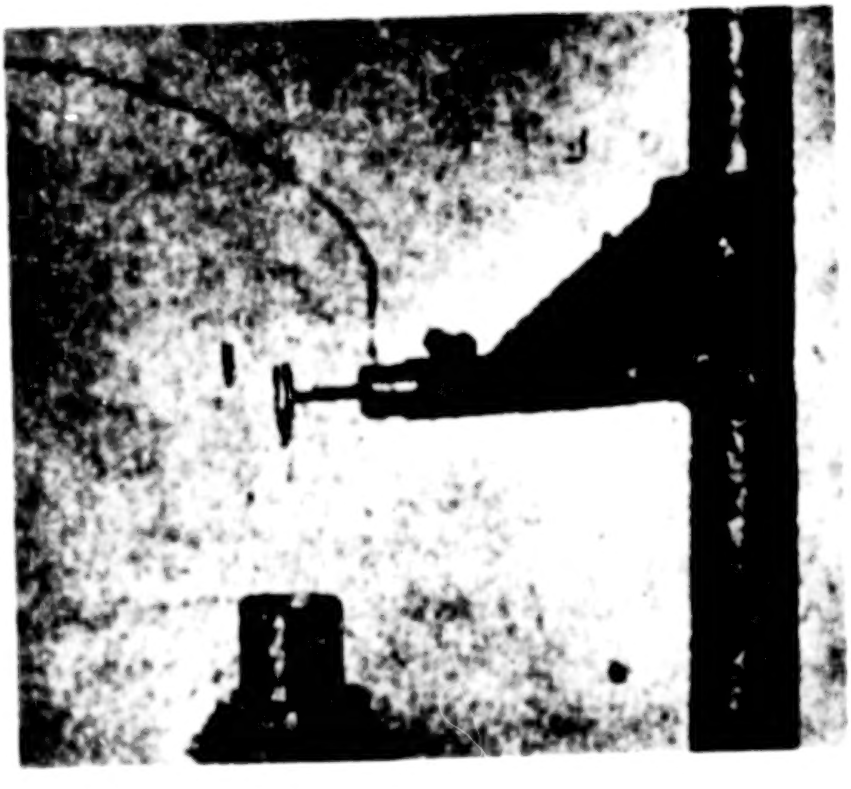


FIG.

rockets of different types were soon put into service by the Red Army. However, the accuracy of rocket missiles remained considerably inferior to that of conventional artillery; it was the most vulnerable point of this type of weapon. Naturally, a suggestion arose: use rocket missiles for blanketing fire. To meet that need, the section of I. I. Gvay (RNII) began to develop multibarrel rocket units. Thus, the famous "Katyusha," used extensively during World War II, appeared (Figure 3).

One important aspect of the practical application of solid-propellant engines involved the jet-assisted take-off of aircraft--a concept which had been tested earlier in GDL under the direction of V.I. Dudakov. In RNII the jet-assisted take-off of the bomber TB-1 was fully developed (Figure 4), reducing the takeoff distance 20-to-25 percent. V.I. Dudakov's section also developed jet catapults.

The studies of solid-propellant rockets conducted by RNII placed the USSR ahead of other great powers in this field. The war demonstrated that the quality of German solid-propellant rockets was considerably inferior, in spite of the efforts in rocketry of some prominent German scientists.

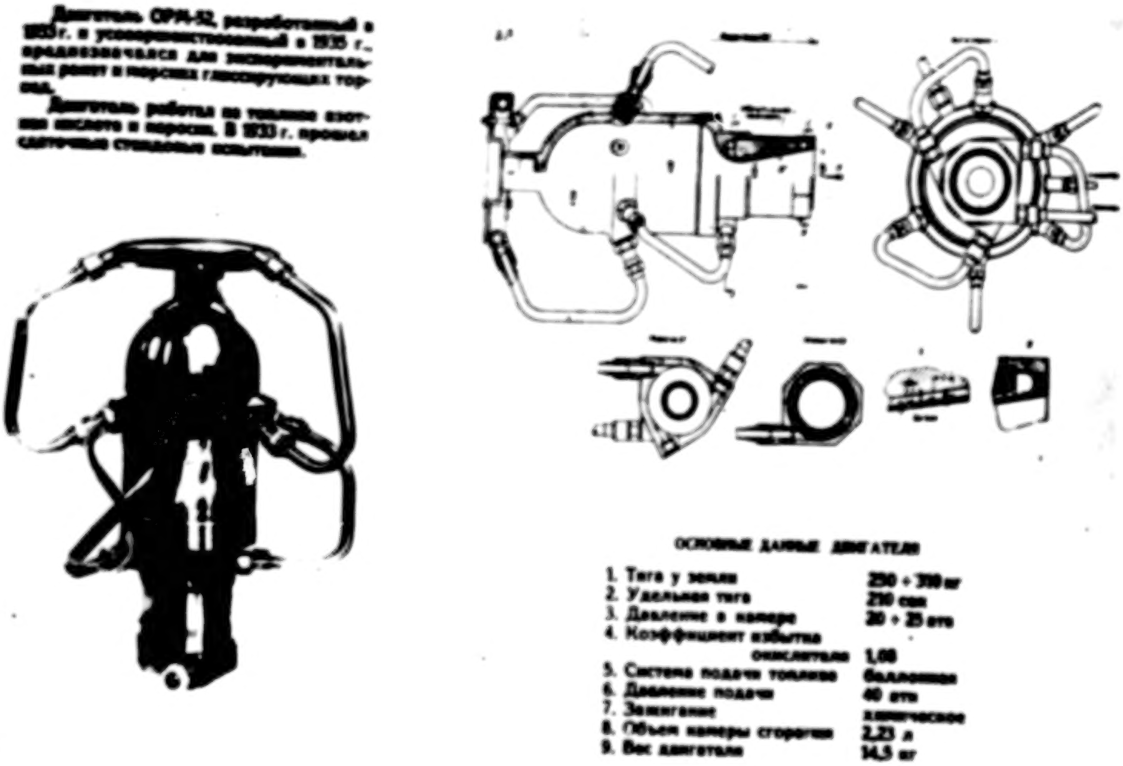
LIQUID-PROPELLANT ROCKETS

Another important trend in RNII activity was the development of efficient liquid-propellant rocket engines. These studies involved nitric acid engines (V.P. Glushko), and oxygen engines (M.K. Tikhonravov, L.S. Dushkin and others). Very large, systematic studies were undertaken to improve the performance and reliability of nitric acid engines and to improve design methods. Among other engines, the ORM-65 (170 kg thrust), and the ORM-52 (300 kg thrust) (Figure 5) should be noted. In spite of numerous difficulties and failures (detonation explosions at engine start, for example) the performance of these engines were brought up to the design values. Nitric acid engines were developed further in 1938-41. In this period the RDA-1-150 (150 kg thrust) and D1A-1100 (1100 kh thrust) engines were manufactured. The high level of development achieved with this type of engine is substantiated by the fact that they were installed and operated practically in all of the prototype vehicles developed at RNII in the last pre-war years (the rocket plane RP-318, ballistic rockets 521 and 604, the rocket fighter BI-1, and others).

Numerous important results were also obtained in the field of oxygen rockets. In particular, it is of interest to note that the development of the modified engine OR-2 (100 kg thrust), which had been started by F.A. Tsander. Although work terminated in 1934, this engine was installed in the winged rocket 216. The development of the engine series 12K (300 kg thrust, Figure 6) was initiated in 1934 by the specialists led by L.S. Dushkin. Engines of this type were installed in the rocket "Aviavnto." Figure 7 shows the engine 205 (100 kg thrust), developed in 1936-1937. Although development of oxygen engines stopped in the last pre-war years--and during the war--it resumed again after the war when the success of oxygen rockets was of world-wide significance.

Fig. 3

Fig. 4



Двигатель ОРМ-52, разработанный в 1933 г. и усовершенствованный в 1935 г., предназначался для экспериментальных ракет и морских глиссирующих торпед.

Двигатель работал на топливе азотная кислота и керосин. В 1933 г. прошел сдаточные стендовые испытания.

ОСНОВНЫЕ ДАННЫЕ ДВИГАТЕЛЯ

1. Тяга у земли	250 ÷ 300 кг
2. Удельная тяга	210 сек
3. Давление в камере	20 ÷ 25 ата
4. Коэффициент избытка окислителя	1,00
5. Система подачи топлива	баллонная
6. Давление подачи	40 ата
7. Зажигание	химическое
8. Объем камеры сгорания	2,23 л
9. Вес двигателя	14,5 кг

Fig.

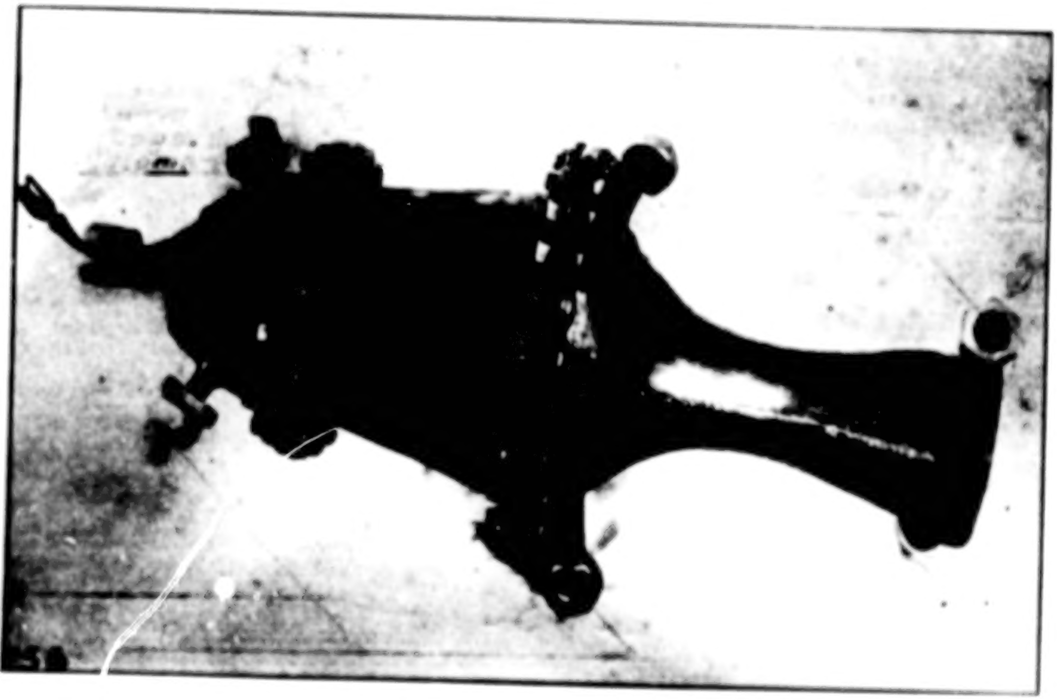


Fig. 6

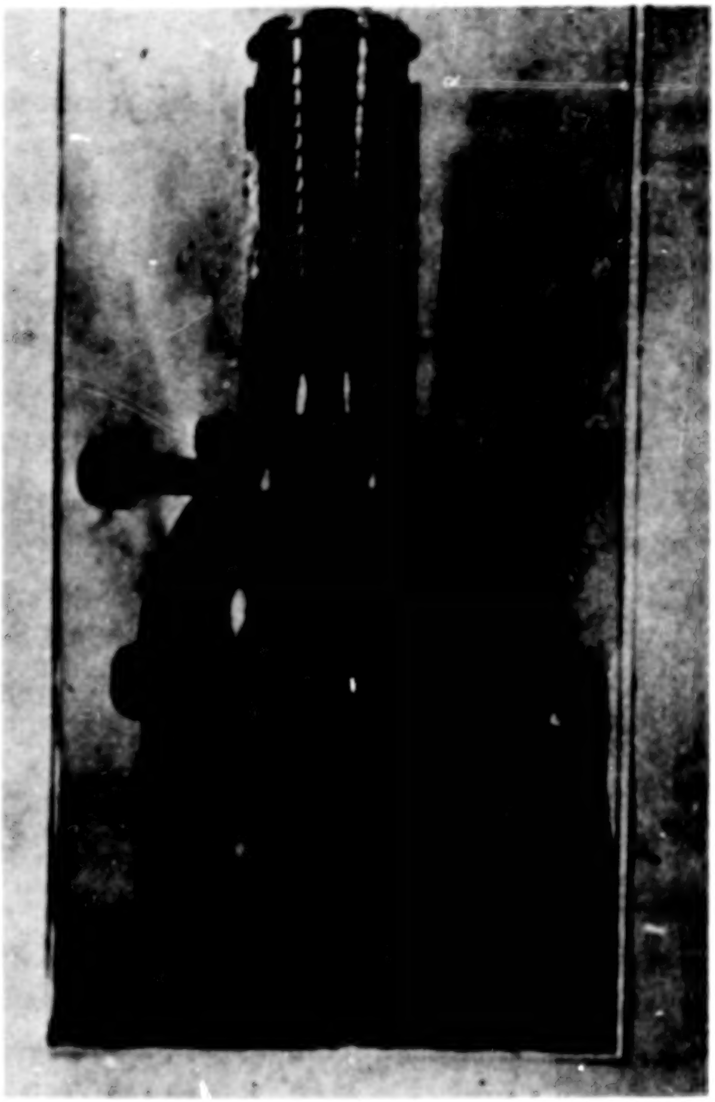


Fig. 7

BALLISTIC LIQUID-PROPELLANT ROCKETS

Work on ballistic rockets, or "long range rockets" (as they then were known) in RNII, was of an exploratory nature. Besides the oxygen rocket 09, which had been first launched by GIRD, the flight tests of the rockets 07 and 10, the rocket "Aviavnto" with the engine 12k (Figure 8), and others, were realized in the RNII by M.K. Tikhonravov and his colleagues. In 1939-40 ballistic rockets 521 and 604 using the combined solid-

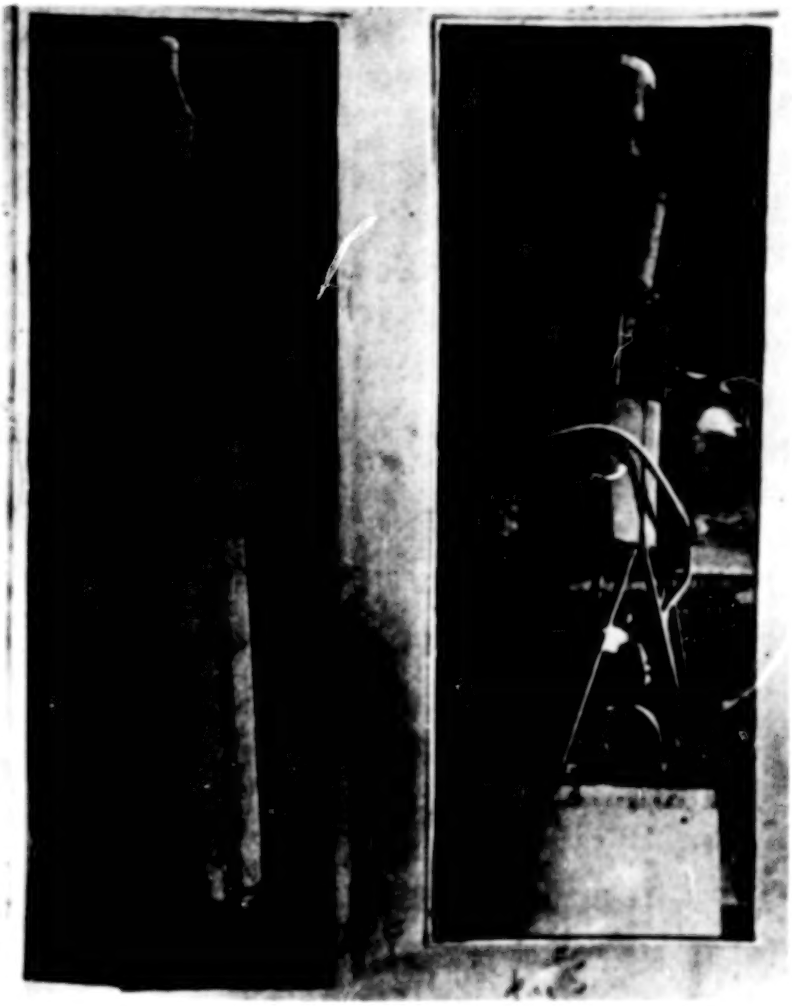


Fig. 8

propellant and nitric-acid engine[+] (about 100 kg thrust) were developed and tested by RNII (Figure 9). Ranges of 19-20 km were obtained in these tests. It should be noted that not only single rockets were tested, but 8-14 rockets were tested in series. Unfortunately, these promising investigations ceased at the beginning of the war because the funds needed to improve the accuracy and reliability of this type of rocket were unavailable.

[+] Grains of solid fuel were introduced into the combustion chamber of the nitric-acid engine; upon ignition of the grains, a short but strong thrust impulse occurred. The grains having burned out, the nitric-acid and kerosine were introduced automatically into the combustion chamber and the engine began to work as a liquid-propellant one.

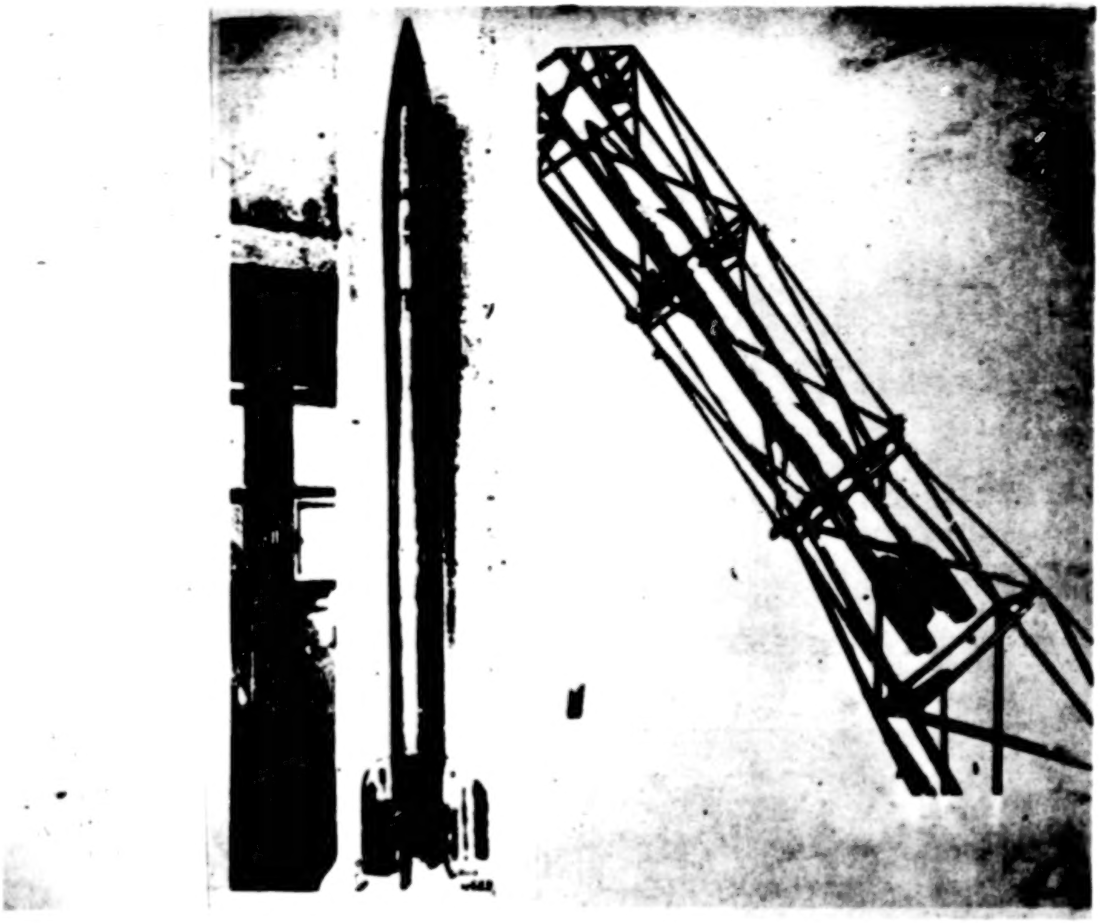


Fig. 9

WINGED LIQUID- AND SOLID-PROPELLANT ROCKETS

Over the period of 1933-39, RNII also developed winged liquid- and solid-propellant rockets. The main purpose here was to investigate the flight dynamics of winged jet vehicles with a large thrust/weight ratio, and the practical application of these rockets in air defense and ordnance systems (computations showed that for a short flight range the initial weight of a winged rocket was lower than that of the ballistic one).

In 1933-34 the rocket 216 (80 kg initial weight), powered by the liquid oxygen engine 02 (OR-2) and equipped with autopilot GPS-2, was developed under the guidance of the author of this paper. A drawing of this rocket is presented in Figure 10. The gyroscope autopilot GPS-2 developed in RNII by S.A. Pivovarov controlled the elevator and ailerons. For the first time in the USSR, initial acceleration of the rocket 216 at launch was provided by a solid-propellant rocket catapult. A drawing of the catapult launching truck appears in Figure 11. Five 216 rockets were tested, but their trajectories and ranges were far from the design values because of the low reliability of the subsystems.

Specialists paid considerably more attention to the reliability of the subsequent winged rocket 212, developed by S.P. Korolev. The model of this rocket is shown on the launching truck in Figure 12. The nitric acid engine ORM-65 was installed in this

Fig. 10

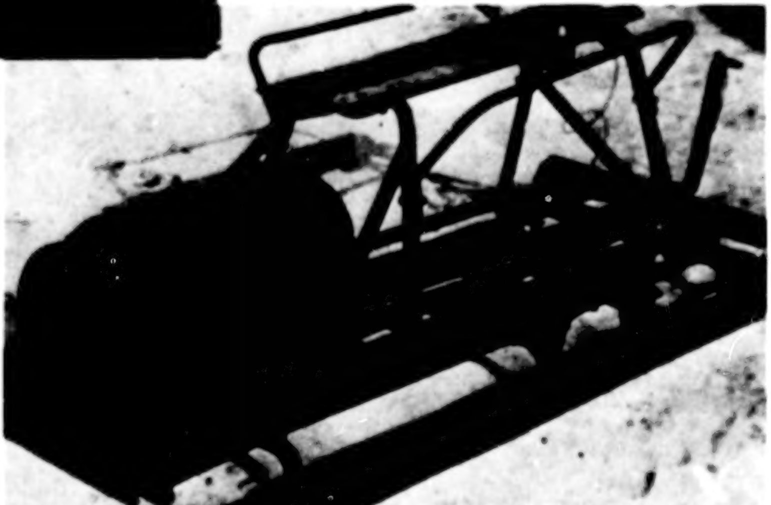


Fig. 11

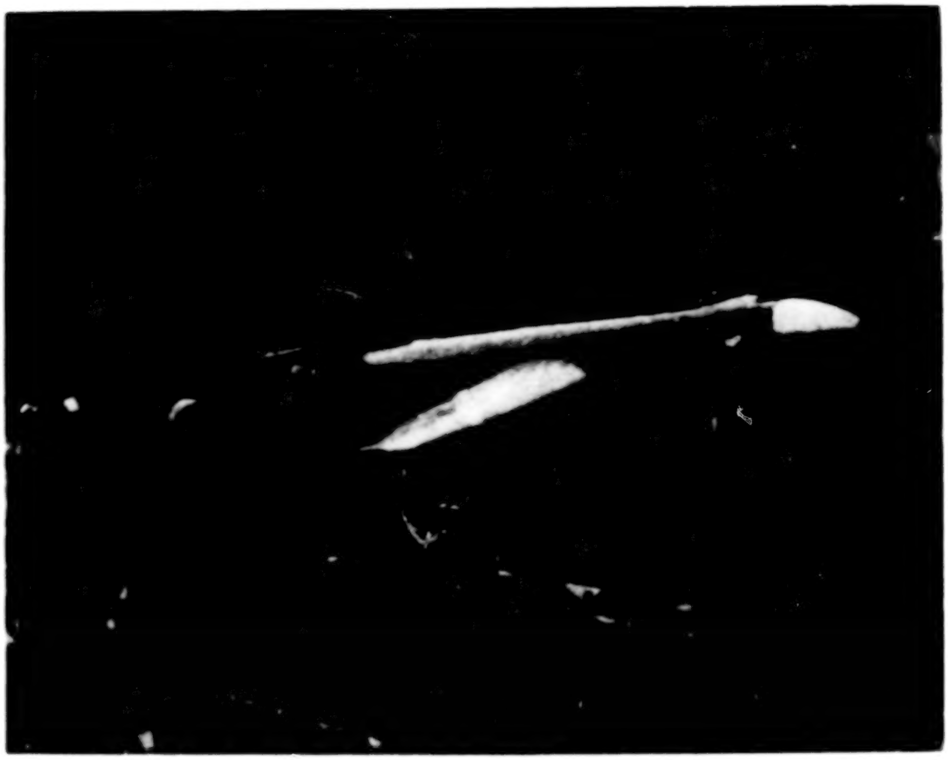


Fig. 12

rocket, which was equipped with the autopilot GPS-3 to control the elevator, rudder, and ailerons (Figure 13). Numerous bench tests to adjust the systems of the rocket 212 were carried out. Unfortunately, only two rockets were prepared for launching in 1939. It could not be expected, of course, that the rocket's performance would be brought up to design values by launching such a small "series" of rockets.

In addition to winged rockets with liquid-propellant engines, air defense winged rockets with solid-propellant engines were developed in 1935-38 under the guidance of M.P. Dryazgov. Winged rockets of different aerodynamic configurations were tested (Figure 14). Better stability characteristics were obtained for the four-winged rocket 217/II with a low aspect ratio. In 1937-38 RNII conducted negotiations with a specialized experimental design bureau for the radio-controlled autopilots and homing heads which were supposed to be installed in these rockets. In 1938 the theory of controlled winged rocket flight developed by M.P. Dryazgov appeared.

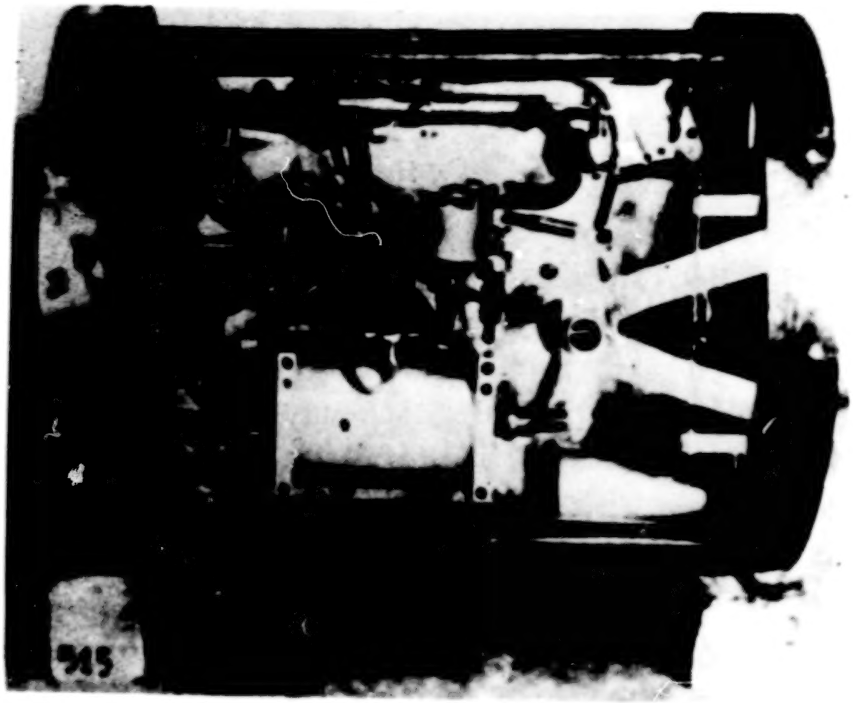


Fig. 13

Fig. 14

ROCKET PLANES

Studies related to rocket-powered aircraft were first conceived in GIRD. There, F.A. Tsander and S.P. Korolev developed the rocket glider RP-1; however, it was not possible to realize this project then. RNII renewed more detailed studies of these aircraft. S.P. Korolev and the author of this paper carried out theoretical investigations in 1936 that showed the rocket-propelled aircraft RP-218, powered with a cluster of three OPM-52 engines, had a new quality compared to the usual propeller-driven aircraft, namely, a very swift rate of climb. After the Korolev report had been heard at a session of the RNII scientific and technical board, officials decided to implement the flight of the man-carrying rocket glider SK-9, named RP-318 (rocket plane 318, Figure 15) as the first phase of the rocket flighter program. The OPM-65 engine was at first chosen for this glider. Numerous detailed ground tests of power plant were performed, including tests of electrical and control systems, fuel system, etc. On the basis of the experience obtained, the new engine RDA 1-150 was manufactured. In October 28, 1940 the first flight in the USSR of a manned rocket plane with this liquid-propellant engine was realized by pilot Fedorov. Figure 16 shows the experimental characteristics of this engine installed on RP-318.

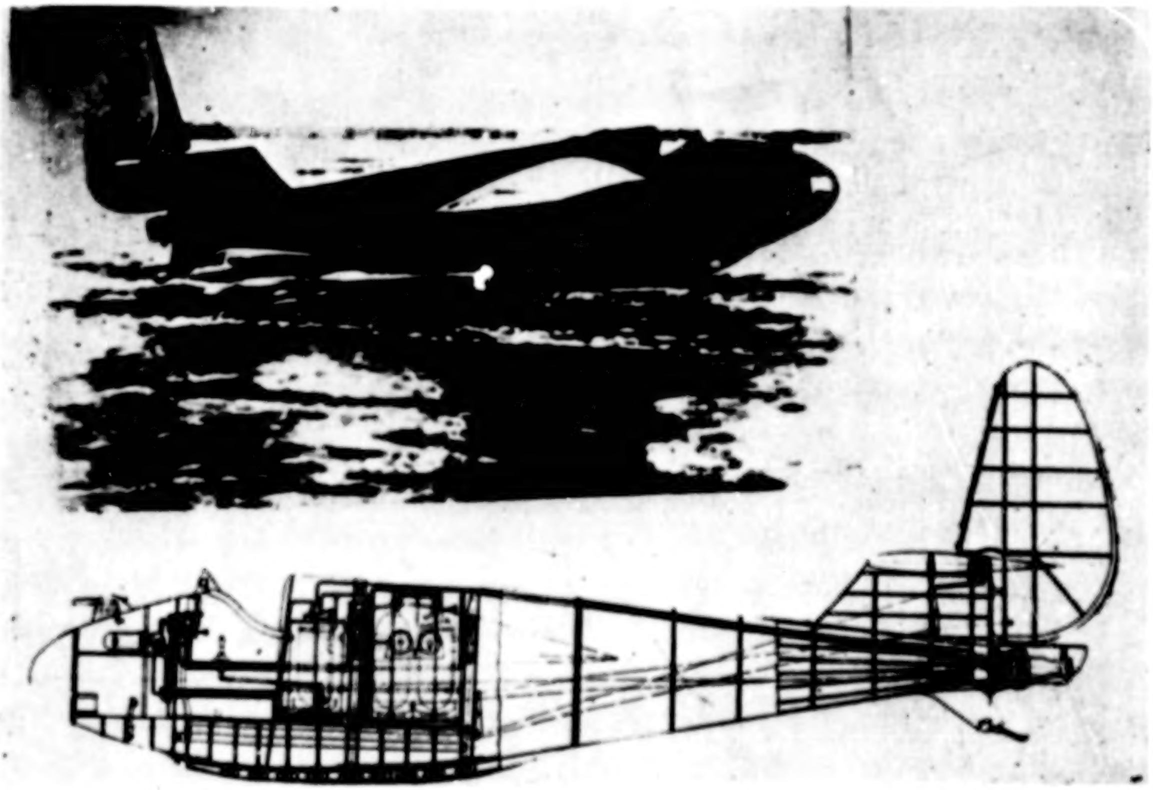


Fig. 15

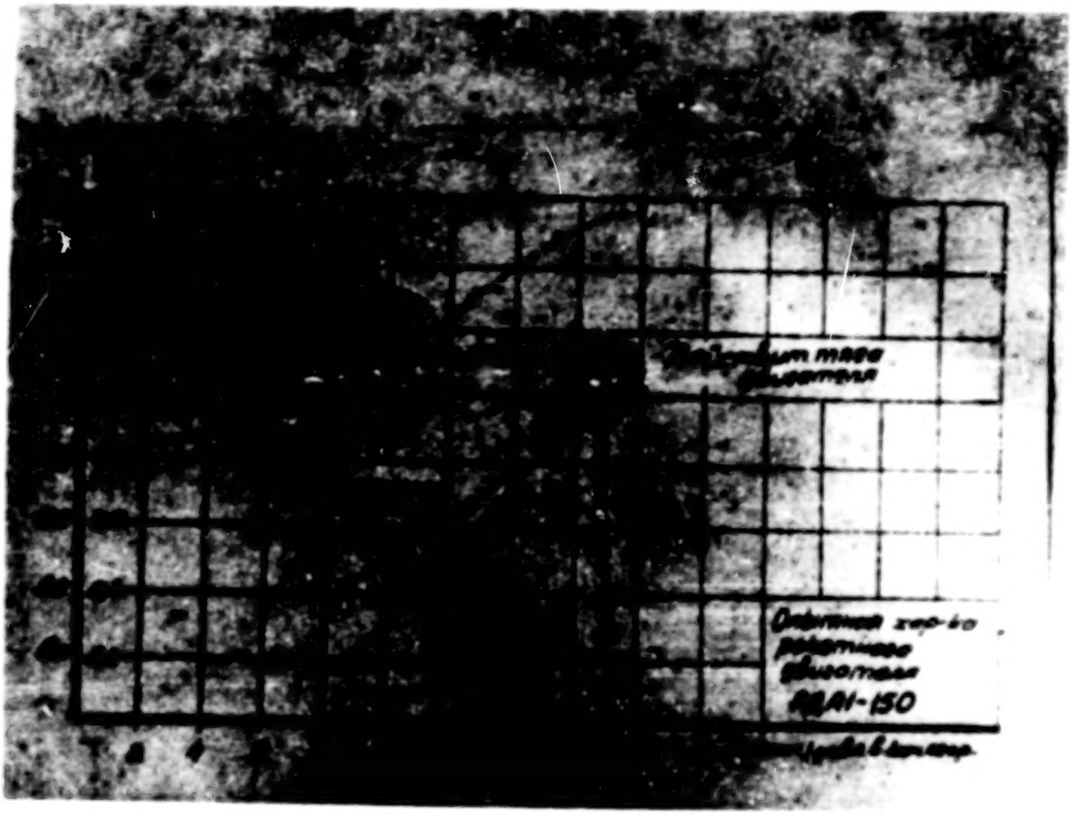


FIG. 17

Development of the rocket fighter BI-1, [illegible] (Figure 17), continued developments in this direction. The [illegible] engine, also developed in RNII, was installed in this aircraft. [illegible] made the first flight of the BI-1 on May 15, 1942. However, an attempt to increase the flight speed resulted in his death. Among the factors which may have been responsible for this catastrophe was the poor knowledge of the air compressibility effect on aircraft controllability; in particular, the dangerous shift of the center of pressure rearward was not yet known.

CONCLUSION

This review of the basic trends of RNII activity and the [illegible] work carried out in 1933-41 is far from complete. In particular, the [illegible]

Fig. 17

engines, liquid- and solid-propellant gas generators, automatic control systems for jet vehicles, etc., have not been considered. What were the main results of this RNII activity in the period of 1933-41? Evident success was achieved in the field of rocket missiles ("Katyusha"), and jet-assisted take-off of aircraft, as the design parameters were realized; these systems played an important role during World War II. Though other projects were not completed, valuable scientific and technological results were obtained in the process of the research and development. A great reserve of knowledge for future studies was provided, experience gained in the field of production technology, and design and test techniques related to various jet vehicles and engines perfected.

But perhaps the most valuable result of RNII pre-war activity was the training of scientific and technological cadres that later brought the unprecedented progress in Soviet rocket technology after the war. We mean specifically the mechanics and test engineers, designers and mathematicians, scientists and project managers.

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ON THE HISTORY OF THE DEVELOPMENT OF SOLID-PROPELLANT ROCKETS IN THE SOVIET UNION[+]

Yuri A. Pobedonostsev (USSR)[++]

Toward the end of the 19th century, solid-propellant rockets were eclipsed by rifled artillery in military applications, lost there former significance, and became used only for secondary purposes such as illumination, signaling, and fireworks, etc. This occurred because of the lack of a scientific theory explaining rocket propulsion, the low effectiveness of the only propellant then used in rockets—gunpowder—and the lack of advance in the design of existing rockets that had hardly changed in the course of centuries. But the principal advantages inherent in the rocket remained: simplicity of design and especially the simplicity and ease of mounting for launching. These considerations constantly attracted the attention of inventors throughout the world. In Russia, work directed toward improving powder rockets never ceased for even a year.

After K.E. Tsiolkovsky created the foundations of rocket-dynamics, the intuitive attempts of inventors to use a propellant with a higher heat energy than gunpowder received a scientific grounding. N.I. Tikhomirov and I.P. Grave independently proposed to use smokeless powder in rockets before World War I. V.A. Artem'ev, I.V. Volovsky, N.V. Gerasimov and others were responsible for a series of interesting proposals to improve the designs of rockets and their launching mounts. But not one of these proposals received official support in Russia.

After 1917 the attitude toward inventors in our country changed sharply. Even during the Civil War, engineers N.I. Tikhomirov and V.A. Artem'ev started work on the design of a smokeless powder rocket and in 1921 a special laboratory was set up for these studies in Moscow. First they tried to work out a rocket engine design that would function according to existing ideas of smokeless powder artillery, but soon they became convinced of the necessity of creating a special, slow-burning rocket powder, realized in the form of a solid composite charge.

[+] Presented at the Fifth History Symposium of the International Academy of Astronautics, Brussels, Belgium, September 1971.

[++] Co-founder of the Group for the Study of Jet Propulsion, Assistant Chief Engineer at the Scientific Research Institute No. 3 in Leningrad, 1935-1946, Professor of aerodynamics, Moscow Aviation Institute 1957-.

In 1924, O.G. Filippov and S.A. Serikov, pyrotechnic specialists working at the Artillery Academy then located in Leningrad, developed a formula for a new type of powder based on a non-volatile solvent 76.5% by weight of nitrocellulose, 23% TNT, and 0.5% centralite to retard burning. It was called "PTP", i.e. pyroxyline TNT powder. By 1928-1929, the work conducted in the Artillery Academy on the new rocket fuel formula and its technology permitted development of a semi-production technique for preparing the solid composition PTP by pressing the prepared powder mass into hollow, preheated female dies.

Preparation of long rods of the solid composite charges of PTP, each with a diameter of 24 mm, for Tikhomirov's Moscow laboratory, was organized in the powder workshops of the Port of Leningrad using the very same presses on which many years before the great Russian chemist D.I. Mendelyev had prepared samples of smokeless powder for the first time. In the mid-1920s Tikhomirov's laboratory was transferred to Leningrad where, during the course of several years, static tests were conducted on composite propellants of various sizes. V.A. Artem'ev obtained positive results in static tests of larger propellant charges made up of five rods compressed together with a combined diameter of 76 mm, with "even" burning over the entire surface. A rocket was soon designed using these solid composite charges and it was successfully tested on March 3, 1928. An ordinary mortar was used to launch the rocket and establish its trajectory in the first 300 meters. This rocket traveled 1,300 meters overall, using half the rocket charge. This was the first Soviet smokeless powder rocket. V.A. Ventsel' and M.B. Serebrjakov also supplied valuable help, working out theoretical questions and solving practical problems of internal and external ballistics.

Having achieved the first promising results, N.I. Tikhomirov and V.A. Artem'ev determined to apply this knowledge toward broadening the scope of their work. A few months later, in June 1928, Tikhomirov's laboratory was reorganized as the Leningrad Gas Dynamics Laboratory (GDL). G.E. Langemak began work at GDL in 1928. He was placed in charge of one of the first priority tasks at the laboratory--the study of the characteristics of burning of powder composite charges in rocket chambers with nozzles. Thereafter he contributed a great deal to the design theories of powder rockets and the methods of calculating rocket loading with charges of smokeless powder. The basic principles such as the burning of rocket charges were first advocated and confirmed in experiments he conducted.

In 1929 B.S. Petropavlovsky was appointed director of experiments at GDL. He soon became the main scientific director of the laboratory. His name is connected with the rejection of the "active-jet propulsion principle" and the switch that led to actually broadening the application of powder rockets designed in GDL. Under Petropavlosky, the basic 24 mm solid composite charges made from PTP, which were prepared in the laboratory shops in great quantities, were selected as the standard in GDL. Therefore, on this standard engineers developed three basic sizes of scaled-up rocket chambers of 68, 82, and 132 mm calibers. The latter two subsequently became the basic calibers of Soviet rocket missiles for decades: the RS-82 and the RS-132, later named "Katyusha."

Various types of solid rocket projectiles were developed on the basis of the experiments and theoretical research conducted in GDL. The main consideration was stability in flight, and the rockets were designed to revolve around their longitudinal axis, maintained by an outflow of gases through a tangential nozzle. However, with such an arrangement for stabilization, about 25% of the rocket charge was lost in imparting the necessary rotation to the projectile. Thus, in developing the rocket projectiles, the collective of GDL had tried first of all to solve the problem of flight stability in order to achieve the highest accuracy. Launch of these rockets was accomplished from a tube. The first missiles were designed in GDL with calibers of 82 and 132 mm, and designated for aircraft. But the missile with the smaller caliber was also designated for firing as a hand-operated anti-tank weapon. However, at this time the turbo-rocket missiles could not be applied as weapons because they were still under development.

In 1929, at the suggestion of V.I. Dudakov, work began at GDL on developing solid-propellant rocket engines for accelerating the lift-off of aircraft. After N.I. Tikhomirov's death on March 38, 1930, B.S. Petropavlosky was named head of GDL, and, after December 1932, I.T. Kleimenov. During this period test pilot S.I. Mukhin played a significant role in the test work of GDL; he fired the 82 mm rotating rockets from a Y-1 aircraft and for the first time accomplished the first practical jet-assisted takeoff of the Y-1 and TB-1 aircraft.

But by 1933, tests of these same rockets with various shapes and with a number of different tail assemblies which did not exceed the caliber size showed that it was impossible to maintain a steady flight and obtain satisfactory accuracy. Therefore, in mid-1933, a project involving rockets with tail sections exceeding the size of the caliber of the projectile was undertaken. In order to launch these rockets, special aircraft firing stands had to be created with longitudinal plates strengthened by curved cross-members. Soon, rockets with the new stabilizer design were created. The dimensions of the tail assembly were 200 mm for the 82 mm rocket, and 300 mm for the 132 mm rocket. The first experimental launchings brought completely satisfactory results in accuracy. The RS-82 flew five km, the RS-132 flew six km.

This was as outstanding achievement of the Leningrad Gas Dynamics Laboratory, and determined the direction of future scientific research and experimental design work in the field of solid-propellant rockets in the USSR. Thus, GDL played a significant role in the creation of the basic theories, planning design methods, and testing of rocket engines with the new, more powerful smokeless powder.

In the interest of developing, strengthening, and broadening the work in rocket technology in the future, a move from a small organization to a large scientific research institute with a well-equipped laboratory and experimental base now appeared urgent. Utilizing the two most productive functioning organizations--Leningrad's GDL and Moscow's GIRD--in October 1933 a single Jet Propulsion Research Institute (RNII) was created, and GDL's work in powder rockets and on a series of other problems were transferred to RNII. At this time a final design of the 82 mm and 132 mm rockets was almost completed and brought to the stage of being put into production. Soon, serial production of 100-200 units of each type began. In the years to come, G.E. Langemak, L.E. Shvarts, Yu. A. Pobedonostsev, M.K. Tikhomravov, F.N. Poida, M.S. Kesenko, V.A. Andreev and others worked on powder rockets in RNII.

Aerodynamic testing of rockets was also conducted by the Zhukovsky Central Institute of Aerohydrodynamics (TsAGI), where their stability in flight was tested using various shapes and sizes of tail sections. Launches at the testing grounds were conducted using launch stands of various lengths. Geometrical eccentricity was measured individually for each rocket. The character of the rocket flight was checked by the smoke path and rapid filming. Beginning in 1935, systematic testing of solid-propellant rocket launched from I-15, I-16, and SB aircraft was carried out and returned positive results as to the accuracy of the rocket flight. The organization of work for the first extensive flight tests of the 82 mm rocket on the I-15 fighter was accomplished under the direction of G.E. Langemak. Test pilot G. Ja. Bakhchivandzhi, who later tested the first Soviet aircraft with a liquid-rocket engine, also took part.

In 1937 a new design for launching rockets from aircraft was developed that in the future was even utilized in ground launching stands. Instead of a pair of longitudinal directing plates for each missile, engineers suggested installing a single directing plate for each missile in the shape of a T-slotted trough. The T-shape proposed by Engineer I. Belov, was approved. The T, affixed to the missile, was inserted into the slot on the aircraft wing which served as the guiding rail. The 82 mm rockets (RS-82) were ready for practical use in December 1937, and the 132 mm aviation rockets, in July 1938. The RS-132 was fired from SB aircraft which carried four rockets under each wing, from the I-16 aircraft with two rockets under each wing, and later from the IL-2 aircraft. In August 1939 these aviation rockets were successfully used in a tactical situation on the Khalin-Gol River in Mongolia.

At the beginning of 1937, RNII was given the task of designing a multi-round ground launching stand using the T-slotted trough guiding rails adapted from aviation. Toward the end of 1938 the first models of the self-propelled stands for volley firing of powder rockets were finished. I.I. Gvai directed their development. From December 8, 1938, until February 4, 1939, the first tests of 132 mm powder rockets, fired from a multi-round base, were conducted. These tests showed that the stands were not yet ready as they afforded insufficient range and a low level of accuracy. But at the same time the tests demonstrated the great possibilities of these rockets and their launching stands.

Later in 1939, A.S. Popov, V.N. Galkovsky, and A. Pavlenko submitted a new design for the self-propelled 16-round launching stand. It was called MY-2, and, after serial production began, the BM-13. It consisted of 16 V-shaped trough-type guide rails, placed along the axis of the vehicle and rising over the driver's cab. In its construction, every two guide rails were united at a common base. The launching stand was mounted on the body of a powerful, tri-axle automobile--the ZIS-6. The stand allowed firing of all its missiles in 7-10 seconds. This firing stand was constructed in August 1939, and in September-November factory testing was carried out that showed a high utility factor. The main rocket model fired from the BM-13 stand was called M-13. It characteristics actually exceeded the performance of earlier rocket designs. The rocket M-13 reached 8,470 meters with a payload of 4.9 kg, a propellant supply of 7.1 kg and an engine thrust of 2,000 kg. The rocket had improved aerodynamic characteristics which guaranteed sufficiently high accuracy.

In late 1940 and at the beginning 1941, the M-13 rockets and the BM-13 launching stands underwent final testing and development, and were then turned over for mass production for the Army. The successive accomplishment of the application of these rockets contributed to a considerable extent to rocket technology developed in the USSR in the post-war period. The outstanding results later achieved in our country in the development of rocket technology and in the conquest of space were to a significant extent brought about because of the early success of these rockets.

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ON THE HISTORY OF THE STRATOSPHERIC ROCKET SONDE IN THE USSR, 1933-1946[+]

Mikhail K. Tikhonravov and V. P. Zaytsev (USSR)[++]

We shall call a stratospheric rocket one equipped with instruments and designed to fly into the stratosphere for purposes of scientific research. The idea of using a rocket in this fashion occurred almost simultaneously in 1933 with the launching of the first Soviet liquid-propellant rockets. These initial rockets were designed and built first, to fly, and second, to permit studying the rocket's behavior in flight. Naturally, the possible practical uses were of scant interest to the designer, because engineering goals determined the purpose of these rockets. However, since almost every rocket can be launched vertically and equipped with various instruments, without qualification it is perhaps imprecise to categorize rockets as stratospheric. Therefore, in this essay we shall deal only with those rockets conceived and built for the assignment and technical conditions of stratospheric research. This group of vehicles will also contain meteorological rockets—sometimes referred to as "recording rockets"—that only differed from the stratospheric ones in their altitude ceiling.

The reports and resolutions of the All-Union Conference on the Study of Stratosphere that took place in Leningrad from March 31 to April 6, 1934,[+++] spread the ideas for building a stratospheric rocket. Participants at the conference proposed to investigate the upper atmosphere, emphasizing their interest in the study of cosmic rays at great

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[+] Presented at the Third History Symposium of the International Academy of Astronautics, Mar del Plata, Argentina, October 1969. For further details on the development of Soviet rocketry during this period, see I. I. Kulagin, "Development in Rocket Engineering achieved by the Gas Dynamics Laboratory in Leningrad;" A. I. Polyarny, "On Some Work Done in Rocket Techniques, 1931-1938;" and M. K. Tikhonravov, "From the History of Early Soviet Liquid-Propelled Rockets;" in The First Steps Toward Space: Proceedings of the First and Second History Symposia of the International Academy of Astronautics, Smithsonian Annals of Flight, No. 10, 1974 - Ed.

[++] Tikhonravov, an engineer and specialist in liquid-propellant rocketry, helped build the first Soviet liquid-propellant rocket in the Group for the Study of Jet Propulsion (GIRD) in the early 1930's with Leonid Dushkin and Yuri Pobedonstsev. Zaytsev's professional affiliations are unknown.

[+++] See Reference 1; the resolutions of this conference were published in a separate pamphlet in 1934 at Leningrad.

altitude. The resolution of the technical section noted that "the conference believes it is necessary to concentrate the main efforts on developing equipment for taking instruments into the stratosphere, using [these] rockets as a transition stage towards designing rockets for manned flight."

S. P. Korolev, Yu. A. Pobedonostsev, N. A. Rynin, and A. N. Shtern, among others, lectured at the conference. M. K. Tikhonravov devoted his address specifically to "the use of rockets for investigating the stratosphere." The rocket Tikhonravov considered for this application was the 09 rocket of the Moscow Group for the Study of Jet Propulsion (GIRD). But this rocket had not been designed to investigate the stratosphere, and was only cited as one example of producing a flying prototype. Thus, it will not be dealt with here. Although confining our attention to rockets expressly designed for entering the stratosphere, we shall include the vehicles actually constructed as well as those that were not launched or even completed.

V. V. Razumov's Rocket. The engineer V. V. Razumov and the Leningrad Group for the Study of Jet Propulsion designed one of the first meteorological rockets. This Leningrad group, organized by the Leningrad Society for Assistance to the Defense Aviation and Chemical Construction of the USSR (Osoaviakhim), built the rocket in mid-1937.[+] Figure 1 shows a schematic drawing and a longitudinal section of this machine. The following numbers indicate its parts: 1 - body, 2 - nose cone, containing a parachute and

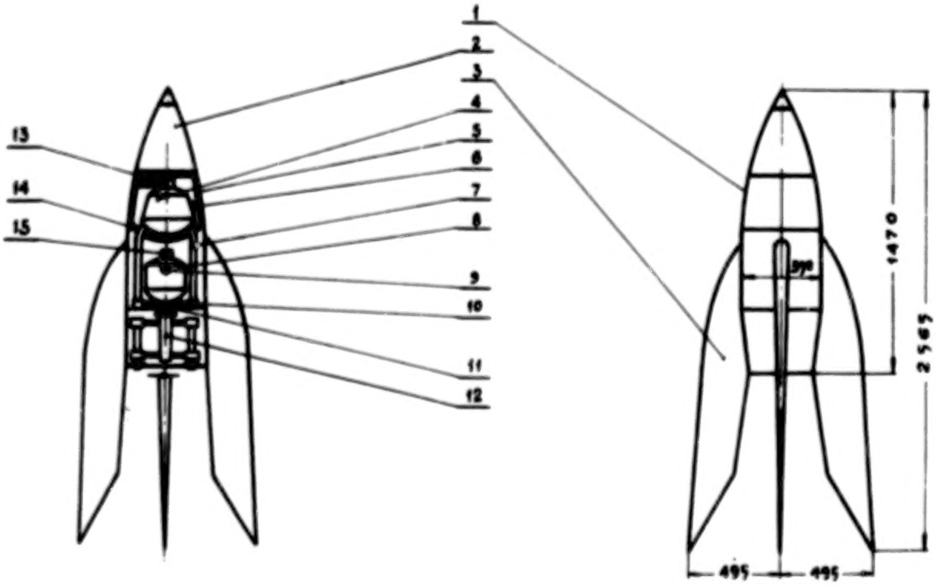

Fig. 1

[+] A short description of this rocket is contained in an article by N. Rynin [Reference 2], in an article by M. K. Tikhonravov [Reference 3], and also in the newspaper Leningradskaya Pravda [Reference 4].

instruments, 3 - stabilizers, 4 - liquid oxygen tank, 5 - insulation, 6 - liquid oxygen fueling valve, 10 - liquid oxygen shutoff cock, 11 - gasoline tank pre-valve.

The rocket featured an engine designed by A. N. Shtern. One of the rotary-reaction variety, Shtern's choice for an engine design was one attempt to solve the problem of efficiently supplying propellant to the combustion chamber. It consisted of pipes fixed along brackets, through which propellants passed, with the rocket engines attached at the pipe ends. The engine nozzles were cut obliquely so that the reactive force had a component in the horizontal plane, directed perpendicularly to the bracket. With the brackets and engine attached to a bearing on the vertical axis, a rotary system was developed in which centrifugal force supplied propellants to the engine. Figure 2 shows the schematic diagram of this system. Apart from solving the propellant supply problem, it was possible to rely on the gyroscopic effect of the gyrating mass, useful from the standpoint of maintaining stability.

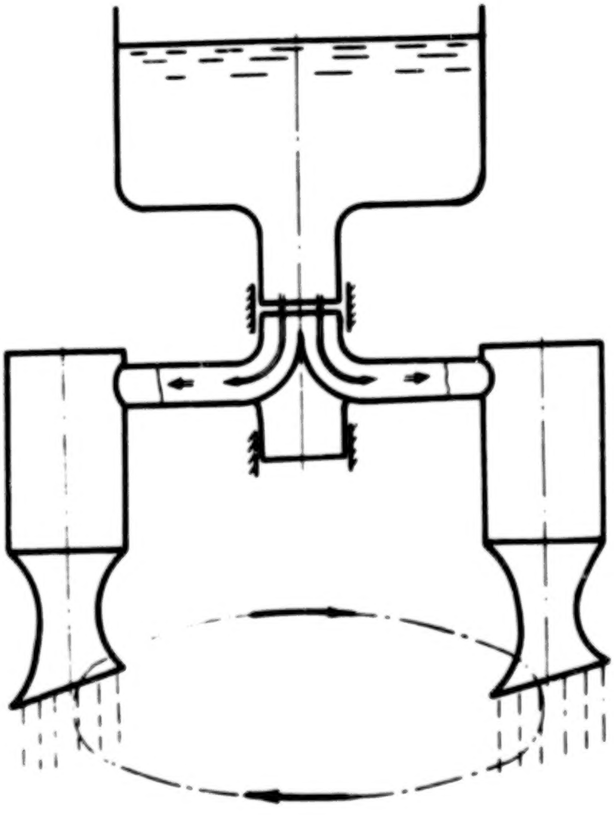

Fig. 2

Shtern designed this engine at the Air Equipment Design Bureau in the Leningrad Society for Assistance to the Defense, Aviation and Chemical Construction of the U.S.S.R. Calculations indicated that the engine could deliver a thrust of 200 kg with an exhaust gas velocity of 2000 m/sec. The maximum estimated speed of the rocket was 100 m/sec; the estimated altitude attainable, 5 km.

The Razumov rocket weighed 90 kg: the construction, 36.2 kg (structure 20.2 kg, engine 16 kg); propellants 22.39 kg (gasoline 4.89 kg, oxygen 17.5 kg); and a payload of 31.41 kg. In this way, the propellant weight amounted only to 25 percent of the rocket's gross weight, or 38 percent, not including the payload. In 1934 the rocket (Figure 3), its drawings and the individual engine components produced by Shtern (a combustion chamber and nozzle), were exhibited during the All-Union Conference on the Study of the Stratosphere. But the building of a complete engine for the rocket dragged on until March 1935, when serious complications caused this effort to be abandoned. However, Razumov's rocket was eventually launched successfully with a solid-propellant powder engine at the Aerolitic Institute station in Slutsk.

Fig. 3

At the beginning of March 1935, in Moscow, a conference on jet propulsion was held on the initiative of the All-Union Aviation, Scientific, Engineering and Technical Society. There, V. N. Vetchinkin, S. P. Korolev, V. P. Glushko, Yu. A. Pobedonostsev,

M. S. Kisenko, and A. V. Zagulin gave lectures. During the conference on March 2, M. K. Tikhonravov also presented another lecture on "the use of rockets for investigating the stratosphere,"[+] in which he showed the possibility of using existing rocket technology to attain altitudes of up to 60 km.

Let us note that the following organizations were interested in or actually developing stratospheric rockets:

1) The U.S.S.R. Academy of Sciences;

2) The Jet Propulsion Research Institute (RNII) of the People's Commissariat of Heavy Machinery Manufacture (NKTP), founded in 1933 on the base of the Moscow GIRD and the Leningrad Gas Dynamics Laboratory (GDL);

3) The Society for Assistance to the Defense, Aviation and Chemical Construction of the U.S.S.R. (Osoaviakhim);

4) The All-Union Aviation, Scientific, Engineering and Technical Society (1932-1941).

To be sure, most of the rockets were built to advance space technology in one way or another, and were not specially designed for investigating the stratosphere. But all of them remain of great interest for the history of the development of jet propulsion in the Soviet Union.

The RNII Rocket. Of the rockets produced by the RNII, the one designed by engineer V. S. Zuyev between 1933-1934 is perhaps the most interesting from the standpoint of studying the stratosphere. The Zuyev rocket was specifically designed for a vertical climb up to 50 km. Shaped like a cigar with four fixed stabilizers on the tail, it had tanks, a body, and tail unit. But it possessed no engine or propulsion unit; eventually the rocket's components were used in another high-altitude rocket. Later on, a rocket of the Zuyev design was built incorporating an 02 engine, and it made flights.

A. I. Polyarny's Rocket. A group of public-spirited persons interested in rocket technology formed a special group within the Society for Assistance to the Defense, Aviation and Chemical Construction of the U.S.S.R. in Moscow, and joined in building a rocket engine designed by engineer A. I. Polyarny, who began working with this group in the autumn of 1934. With limited resources, Polyarny designed a relatively simple meteorological rocket (Figure 4). By the spring of 1935 the rocket and its engine had been constructed, and a test firing made at Nakhabino, near Moscow; the flight test, however, proved unsuccessful. The rocket, with its engine ignited, became wedged in the launcher and operated until the propellant was consumed. This occurred because a special key that opened the tank valves was not removed quickly enough.

The Polyarny rocket engine utilized alcohol and liquid oxygen, and developed a thrust of approximately 100 kg. Liquid oxygen was supplied by pressure from its own vapors, in the same manner as the 09 motor, with alcohol fed under pressure previously created in the fuel tank. In time, this rocket was given to Design Office No. 7 where

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[+] Published in the Collection of articles in Reference 5.

Polyarny went to work. In Design Office No. 7, created in the second half of 1935, it became known as the R-06, was modernized, and made many flights. However, by then it had a different role to play.

Fig. 4

The All-Union Aviation, Scientific, Engineering and Technical Society Rocket. In 1935, unused components of some oxygen rockets remained at the RNII. Therefore, institute personnel decided to build a stratospheric rocket using this material. The All-Union Aviation, Scientific, Engineering and Technical Society supported this initiative and granted 5000 rubles, allowing the work to proceed.

For this rocket the following components were available: a) a 12k ceramic-lined engine designed by L. S. Dushkin, which delivered a thrust of 300 kg for 60 seconds, and operated on liquid oxygen and 95 percent alcohol; b) an ORM-50 Gas Dynamics Laboratory engine that was to have been used in the 05 GIRD rocket designed by M. K. Tikhonravov in the autumn of 1933. It weighed 15 kg and produced a specific thrust of 205-207 kg. For a variety of reasons, however, this latter unit was not used.

Figure 5 shows a section of this rocket. Stabilizers from the 05 rocket were discarded in favor of stabilizers from the RNII rocket, designed by engineer V. S. Zuyev, that had not been completed. These stabilizer wings were contoured and hollow. In this way, the All-Union rocket assumed a launch weight of 100 kg, of which 32 kg consisted of propellants. Engineers estimated that the rocket would reach an altitude of 3800 meters. The nose cone contained a parachute weighing 8 kg, and was equipped with a simplified barograph designed by S. A. Pivovarov for measuring the altitude.

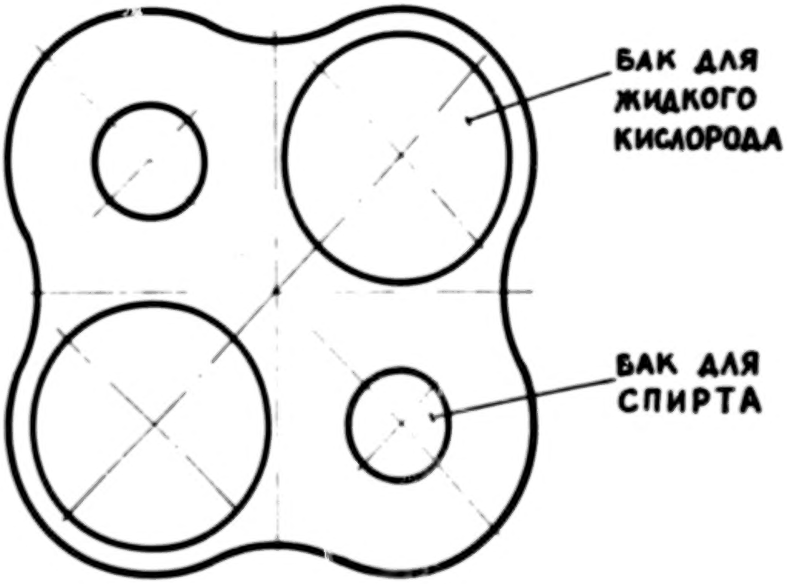

Fig. 5

The first launch of the All-Union Aviation, Scientific, Engineering and Technical Society rocket took place in April 1936 from a Tikhonravov-designed launcher made for the GIRD 07 rocket. However, the rails of this launcher had been lengthened. Pravda featured a report of the launch under the title "A Rocket Goes Into The Air," and carried a photograph of the rocket in the launcher. This is how L. Brontman, the Pravda correspondent, described the rocket's flight: "The mechanic switched on the electrical primer. There was a gray cloud of evaporating oxidizer. A spark. Suddenly, a blinding yellow tongue of flame appeared at the bottom of the rocket. The rocket slowly moved upwards along the launcher guide rails, slipped from their steel clutches and hurtled swiftly upwards. The flight was an unusually effective and beautiful sight. A flame flew from the motor's nozzle, and the gas efflux was accompanied by a deep hollow roar. After reaching a certain altitude, the white parachute in the rocket opened automatically and the machine descended slowly to the snow-covered field."

For further flights, technicians erected a wooden mast similar to 48-meter radio masts, but with a guiding strip made from a rail of a narrow-gauge railway, which held the brackets—the rocket's eccentric clamps (Figure 6). This mast was used as a launcher on August 2, 1937; however, during the launch, the maximum pressure gauge in the tanks failed, forcing a postponement. A few days later, on August 15, a successful launch took place and the rocket rose vertically and almost disappeared from view. When it fell back to Earth, the parachute opened, but became detached and the rocket disintegrated on impact. The instrument for recording the altitude was recovered; it read 2400 meters. Since the instrument was installed inside the rocket, participants assumed that it recorded the altitude at which the parachute opened. The rocket was seen to rise higher after the parachute opened; therefore, there is reason to believe that the rocket reached an altitude greater than 3000 meters.

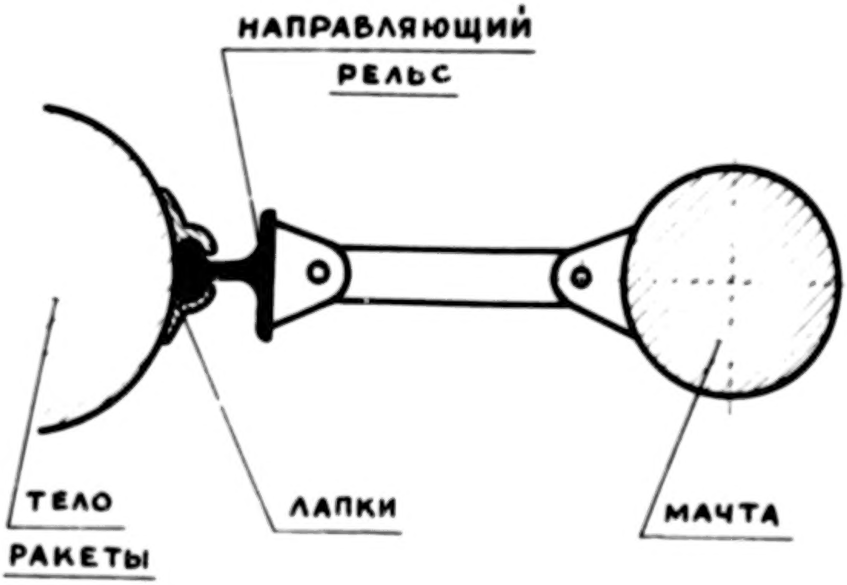

Fig. 6

The A. F. Nistratov Meteorological Rocket. This rocket was built in the workshops of the Scientific Research Institute of the Civil Air Fleet of the U.S.S.R., on funds of 5000 rubles, during 1936 and 1937. Although completed, it was not tested (Figure 7). The engine, made of duralumin, was cooled with water, with the water passed into the combustion chamber. The engine operated on liquid oxygen and oil.

According to calculations, the addition of water, while lowering the temperature, would have little effect on the thrust. This rocket was equipped with a parachute that could be automatically deployed by a powder ejection charge.

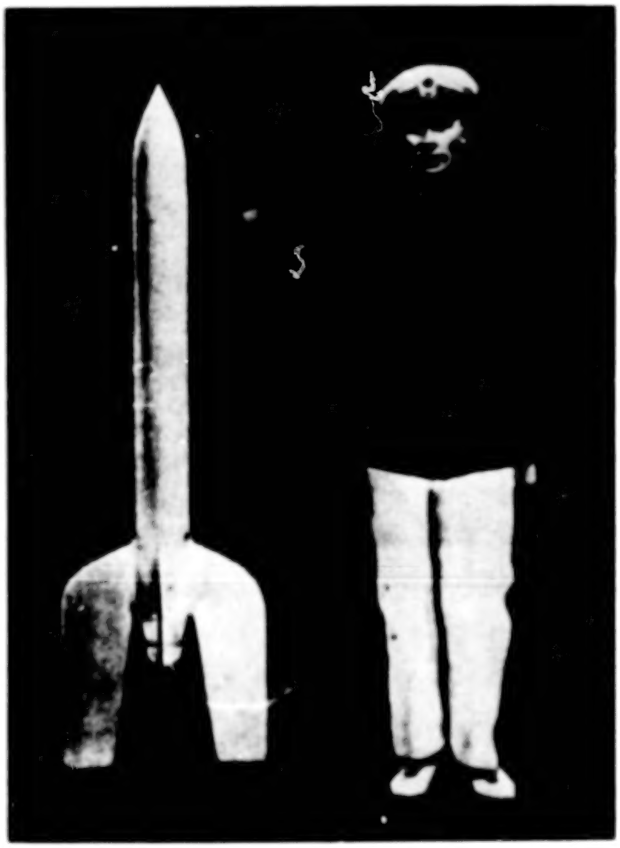

Fig. 7

The L. S. Dushkin and M. K. Tikhonravov Rocket. Construction of this stratospheric rocket, designed in 1937 to reach an altitude of 30 km, was not completed. Nonetheless, models of the rocket were made and tested using black powder motors. The powder motors were old ones, having been supplied to the Czarist army in Russia in 1916, but they sufficed to test the stability of the rocket models. Among various models built, mention must be made of some that arranged the propellant tanks in a torus design. In one version, a tank in the shape of a torus was made and tested for strength, though the rocket itself was not built.

The L. S. Dushkin Rocket. Designed by Dushkin in 1937, this vehicle was built in the following year. It contained a gyroscopic control unit designed by S. A. Pivovarov, and featured automatic control rudders on the stabilizers. The rocket had a removable nose cone with a parachute, an automatics compartment, and a propellant tank section between which was located a propellant-pressure accumulator. This original method of propellant supply had been conceived in 1935. The rocket also used a ceramic-lined 205 engine with a metal nozzle designed by Dushkin that delivered a thrust of 150 kg. This engine, a further improvement of the 12k engine, and its propellant supply system were developed and bench tested. One rocket and four automatic control systems were produced. Since this stratospheric rocket was designed for vertical flight and did not carry out tactical tasks, further finances were unavailable, and it was not produced.

The R-05 Rocket. O. Ku. Schmidt initiated construction of this rocket, with the work supported by the Geophysical Institute of the U.S.S.R. Academy of Sciences. The rocket's altitude ceiling was to be 50 km--and that capability interested the Institute of Technical Physics (Leningrad), where researchers wished to use the vehicle to study space radiation. Towards the end of 1937 Design Office No. 7 accepted the assignment for building the liquid-propellant rocket, designating it R-05.[+] A. I. Polyarny and P. I. Ivanov created a joint project between the Design Office and Geophysical Institute.

In the summer of 1938, P. I. Ivanov carried out special work to stabilize the vehicle with a gyroscope, rigidly connected inside the rocket body. Six models were constructed with small liquid-propellant motors burning ethyl alcohol and liquid oxygen. The model with Ivanov's gyroscope weighed 12 kg. These models flew and gave satisfactory results.

The full-scale R-05 rocket, built later in 1938, incorporated an engine produced by F. L. Yakaytis that operated on liquid oxygen and 96 percent ethyl alcohol. This propellant supply system—a propellant-pressure accumulator--had been developed in 1937. It should be noted that the use of a propellant-pressure accumulator was made possible by the duraluminum propellant tanks. The rocket itself was to be accelerated out of a launch tower by two solid-propellant booster motors. The Yakaytis liquid engine had the following characteristics: thrust, 185 kg with a specific impulse of 210 sec; burning time 25 seconds. The launch weight of the rocket with its solid-propellant boosters was 60.5 kg, and possessed a caliber of 200 mm. The burning time of the boosters was 2.58 seconds; the average thrust of one booster: 200 kg. The capacity of the liquid oxygen tank was 20.5

[+] This rocket should not be confused with the GIRD 05 rocket. The numbering of machines at the GIRD involved a two-digit sequence: 01, 02, 03, etc., without letters. The numbering of the Design Office No. 7 rockets also used a two-digit sequence, but began with the letter R: R-01, R-02, R-03, etc.

liters, and it was filled to 85 percent capacity; the volume of the alcohol tank was 13 liters, and it was filled to 54 percent capacity. The pressure created in the tanks during engine operation was 25-28 atmospheres.

Up to an altitude of 10 km, engineers intended to maintain the vehicle's vertical trajectory by using a source of infrared rays that would react upon photoelectric cells on the stabilizers. The rudders, automatically controlled, would keep the rocket in the narrow infrared beam. In 1938 the Ukrainian Physicotechnical Institute developed the photoelectric cell system for the R-05 rocket. But in April 1939, organizational complications caused all work on the R-05 to be suspended. However, at the beginning of 1940, the Moscow Higher Technical School (MVTU), supporting the initiative of the rocket's developers, agreed to complete the R-05 rocket on the condition that a customer be found. The Hydrometeorological Service at the Council of People's Commissars of the U.S.S.R. soon agreed to finance this work, but the war terminated these efforts a few months later.

The P. I. Ivanov Rocket. In 1943, with the war still underway, the P. N. Lebedyev Physical Institute of the U.S.S.R. Academy of Sciences expressed interest in building a rocket that would carry instruments up to an altitude of 40 km to investigate the intensity of cosmic radiation. The Institute proposed to launch a rocket from the Academy of Sciences' mountain station in Pamir at an altitude of 4000 meters. P. I. Ivanov accepted this assignment, and in April 1944 began the design work. Ivanov directed the calculations and design effort, with the participation of V. V. Abramov, and, later, I. V. Yaropolov. They completed this work in June 1945.

In October 1945, M. K. Tikhonravov, in whose laboratory the design work had been carried out, gave a lecture on the results at the Lebedyev Physical Institute's Technical Council. A few months later, in December 1945, upon the invitation of Academician S. I. Vavilov, Ivanov submitted a report on the project in a meeting at the Institute. In both cases, the project was approved. In March 1946, the Lebedyev Physical Institute of the Academy of Sciences authorized the construction of ten rockets, with delivery by May 15, 1946.

The P. I. Ivanov stratospheric rocket belonged to the type of rocket that involves successive separation of stages. The rocket consisted of three tandem solid-propellant powder rockets (Figure 8) with an internal chamber geometry of a 132-mm caliber rocket shell. The chambers were made from duraluminum and connected in series. A slow-burning pyrotechnic compound compressed in a special tube ensured ignition of the final stages. The external diameter of the rocket was 138 mm; the length of the entire vehicle, 3420 mm. Solid-propellant powder motors were chosen exclusively in order to produce the stratospheric rocket as quickly as possible. Subsequently, Ivanov suggested the development of a liquid-propellant rocket.

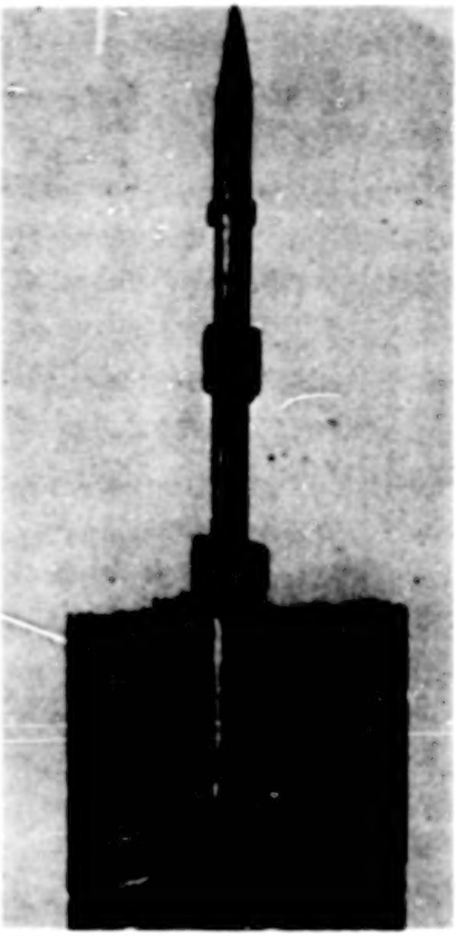

Fig. 8

A solid-propellant starting booster motor, which lengthened the rocket to 4230 mm (Figure 9), was used to increase the speed at which it left the launcher to 100 m/sec. The launcher itself possessed a guiding length of 10.5 meters. The weight of the rocket with its starting booster, completely equipped, was 87.2 kg. Without the starting booster, the three-stage rocket weighed 62.5 kg. After jettisoning of the individual stages, the weight reduced to 41.4 kg, and finally, to 22.1 kg, respectively (with propellant). The weight of the rocket at the highest point of the trajectory was 14.9 kg with instruments.+

+When the war ended, among the German rocket equipment recovered in 1945, there was a Rheinbote solid-propellant, multistage rocket which, except for the propellant charge, hardly differed in overall design from the P. I. Ivanov stratospheric rocket. The dates shown above indicate that the Ivanov rocket was developed quite independently in the Soviet Union.

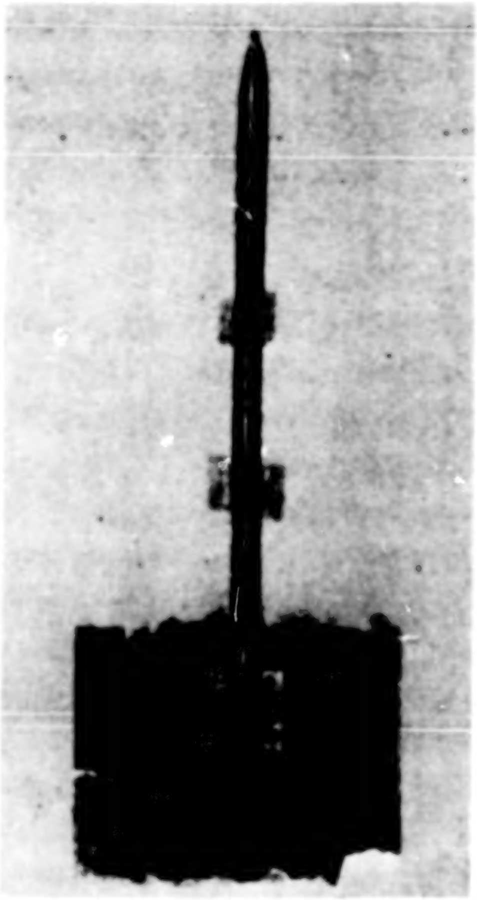

Fig. 9

The rocket could attain an altitude of 48 km on the condition that the launch took place at an altitude of 4000 meters. When launched from sea level, the ceiling would be 35 km. Test firings were made from a proving ground on March 19, 1946, to demonstrate decoupling and stability. A modified rocket employing steel combustion chambers in place of the duraluminum chambers produced satisfactory results.

During the second half of June 1946, experimental launchings were conducted with the Ivanov stratospheric rocket. These tests were conducted for the purpose of an economic evaluation by the P. N. Lebedyev Physical Institute of the U.S.S.R. Academy of Sciences, which was testing special research instrumentation.[+] Three rockets were prepared

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[+] It should be noted that the maximum acceleration during launch was 130g, and the scientific instruments had to withstand this force.

for launch. The combustion chamber of one of them disintegrated at launch; the second changed course erratically when the booster disconnected upon leaving the launch tower; the center of gravity of the third rocket was artificially moved forward, and it flew well.

Radar was employed to determine the altitude attained on the third flight in late June. Probably, this was the first attempt to use radar to determine the trajectory of a rocket. The Lebedyev Physical Institute ceased using the Ivanov rocket at the end of the summer of 1946 because it lacked sufficient power to lift the desired scientific instruments. By this time, however, work on liquid-propellant stratospheric rockets had progressed considerably. These later vehicles reached greater altitudes, and contained heavy, large, modified scientific instruments.

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EXPERIMENTAL RESEARCH AND DESIGN PLANNING IN THE FIELD OF LIQUID-PROPELLANT ROCKET ENGINES CONDUCTED BETWEEN 1934-1944 BY THE FOLLOWERS OF F. A. TSANDER[+]

Leonid S. Dushkin (USSR)[++]

This report describes Soviet research and design planning of liquid propellant rocket engines (hereafter termed LPREs), a field often insufficiently elucidated in the technical literature of the history of rocket technology. The author, a pupil of F. A. Tsander, took a direct part in fulfilling this research in a subdivision of the Jet Propulsion Research Institute (RNII) on a GIRD team headed by Tsander. This report, however, covers only some parts of the history of the development of Soviet LPREs and does not touch upon the research fulfilled in this same period by specialists in other areas of RNII, or in other organizations.

Before moving to the basic topic, I consider it necessary to make a few preliminary observations about the principle characteristics of Tsander's practical work in LPREs insofar as these characteristics have been overlooked in the writings devoted to his work. The name of Tsander is well known in the USSR and abroad as a follower of K. E. Tsiolkovsky and as a great scientist in the field of interplanetary flight theory. He is also known as one of the first research engineers to lay down the foundations for design work on LPREs in the Soviet Union and create the methods for LPRE engineering calculations, projections, and experimental elaborations. His ideas and scientific legacy are thus highly valued in our country and have become the subject of investigation by specialists and historians of rocket technology.

The first conference devoted to the elaboration of Tsander's scientific legacy and the development of his ideas was held in May 1970 in Riga; lectures given by specialists in different sessions investigated his work and clearly demonstrated its importance in the theory and construction of jet engines, in astrodynamics, in life

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[+] Presented at the Fifth History Symposium of the International Academy of Astronautics, Brussels, Belgium, September 1971.

[++] A collaborator of F. A. Tsander and a contemporary of Sergey Korolev, Dushkin specialized in rocket engine design and the development during the 1930s and 1940s in GIRD, RNII, and other scientific institutes.

support systems, and also, the influence of his thoughts and ideas in solving the contemporary scientific-technical problems related to rocket engines and cosmonautics.

Being a theoretician and a space flight enthusiast, Tsander attached paramount importance to questions of the choice of method and the direction and sequence of design work—especially as it related to the problems of LPRE. He imbued these questions with a sense of technical reality and scientific validity and perspective. We know that he created his school in the field of the theory and planning of jet engines on the basis of his own theoretical and practical work. The characteristics of this school, as related to LPRE, appears in detailed examination of his theory of rocket engines,[4] methods of propellant calculations, experimental research on the laboratory model "OR-1,"[2] design decisions in applied oxygen LPREs—developed in the GRID engines "OR-2" and "0-10"[3]-- and also in the studying of schematics and technical data on LPRE developed by Tsander that formed the tasks undertaken in GIRD.

The following points reflect the basic elements of the idea content of this school of thought in LPRE:

1. Strict scientific and technical substantiation of the data for proposed engines,
2. Introduction of experimental research on models and laboratory devices.
3. Application in the LPREs of propellants based on liquid oxygen, liquified gases, and metals,
4. Implementing construction of LPREs as independent hardware, especially in supply, direction, and regulation of the propellant flow,
5. Development of a one-chambered LPRE allowing an increase in thrust regulated over a wide range,
6. Creation of combination rocket engines which united in one power plant various kinds of engines,
7. Technological efficiency, reliability and power increase in the most advanced designs based on subsystem arrangements for ease of dismantling and substitution, during the development of prototype and final construction stages.

The experience realized in the work at GIRD between 1932-1933 strengthened the belief in the validity of the Tsander school, and in the necessity of developing its methods for application to further activity. After Tsander's death in 1933, his school continued to develop and exerted a significant influence on the work in the LPRE field in RNII--created at the end of 1933--in which his students, followers, and supporters elaborated upon the theoretical, experimental, and design directions pioneered by Tsander. In RNII between 1934-1944, reflecting this stage of Soviet rocket engine development, his followers carried out a widespread LPRE research and development program.

In order to give you an idea of the main directions of this work and the results obtained, I have provided below the basic technical data, construction designs, photographs of some of the devices taken from their original sources (preserved in the archives), in the chronological order of their completion.[1-16]

ALCOHOL-OXYGEN LPRE, SINGLE-FIRING "12-K" ENGINE WITH A THRUST OF 300 kg

This engine, designed, constructed, and tested on a stand in 1935-1936, was earmarked for use in wingless and winged rockets. The distinctive features of the "12-K" engine in comparison with other engines created in GIRD were: a higher level of thrust, and pressure in the combustion chamber, as well as a design construction characterized by vortex mixing of propellants, and the use of ceramic fire-proofed materials of aluminum oxide and magnesium oxide for thermal protection of the chamber and nozzle, without the use of external cooling. The "12-K" engine design is presented in Figure 1. The engine's performance parameters were:[4]

1.	Thrust	300 kg.
2.	Pressure inside the chamber	15 atm.
3.	Specific impulse	210 sec.
4.	Pressure of fuel feed	25 atm.
5.	Diameter of sections of the nozzle	42/82 mm.
6.	Volume of the combustion chamber	2 L.
7.	Overall dimensions	450 x 220 mm.
8.	Weight	13.5 kg.
9.	Duration of a single firing	60 sec.

While testing the "12-K" engine, data was obtained close to original calculations. The engine demonstrated a high stability of burning without pulsation of pressure inside the chamber. Therefore, in 1937 the "12-K" engine was placed in the stratosphere rocket sonde "Aviavnito." The first launching took place on April 6, 1936, and was described in Pravda (April 8, 1936); the second and more successful launching on August 15, 1937, employed a 48-meter launching mast.[+] The "Aviavnito" had a starting weight of 100 kg, a diameter of 300 mm, and a length of 3,000 mm. With a load of 32 kg of propellants, the rocket's calculated peak altitude was estimated at 10,800 meters. An overall view of the rocket, installed on the launch stand for the second flight, appears in Figure 2.

[+] See Mikhail M. Tikhonravov and V. P. Zaytsev, "On the History of the Stratospheric Rocket Sonde in the USSR, 1933-1946," in this volume--Ed.

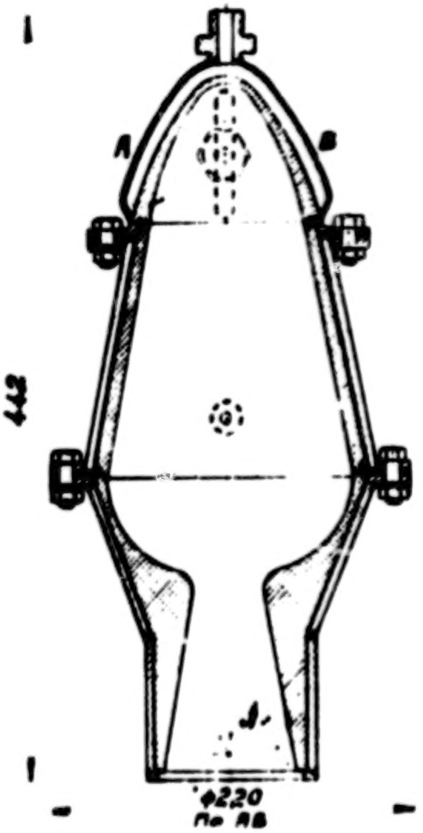

Fig. 1
Schematic of the 12-K Alcohol-Kerosene LPRE

In 1937-1938 research began with the aim of improving the scientific basis of the LPRE engine designs before final construction, to increase the reliability, and to improve the characteristics of recently created models of LPRE working on nitric acid and kerosene. The base for conducting the research was the LPRE Engine Laboratory (at this time part RNII), which was called upon to conduct research on oxygen[5] and nitric acid LPRE, permitted testing of these laboratory models, and propellant combustion tests.[6,7]

Because this research had an applied character, the goal involved studying the more "critical" problems encountered in creating nitric acid LPREs, related to assuring the reliability of the ignition, the stable and complete combustion of propellants during the burning process, and the efficient cooling of the chamber, etc. The initial input and the test results of this research were as follows:

Researching the Processes of Igniting Propellants

The installation consisted of a combustion chamber with a constant volume, with a nozzle and a system for pressure impulse feeding of propellants through atomizers in an

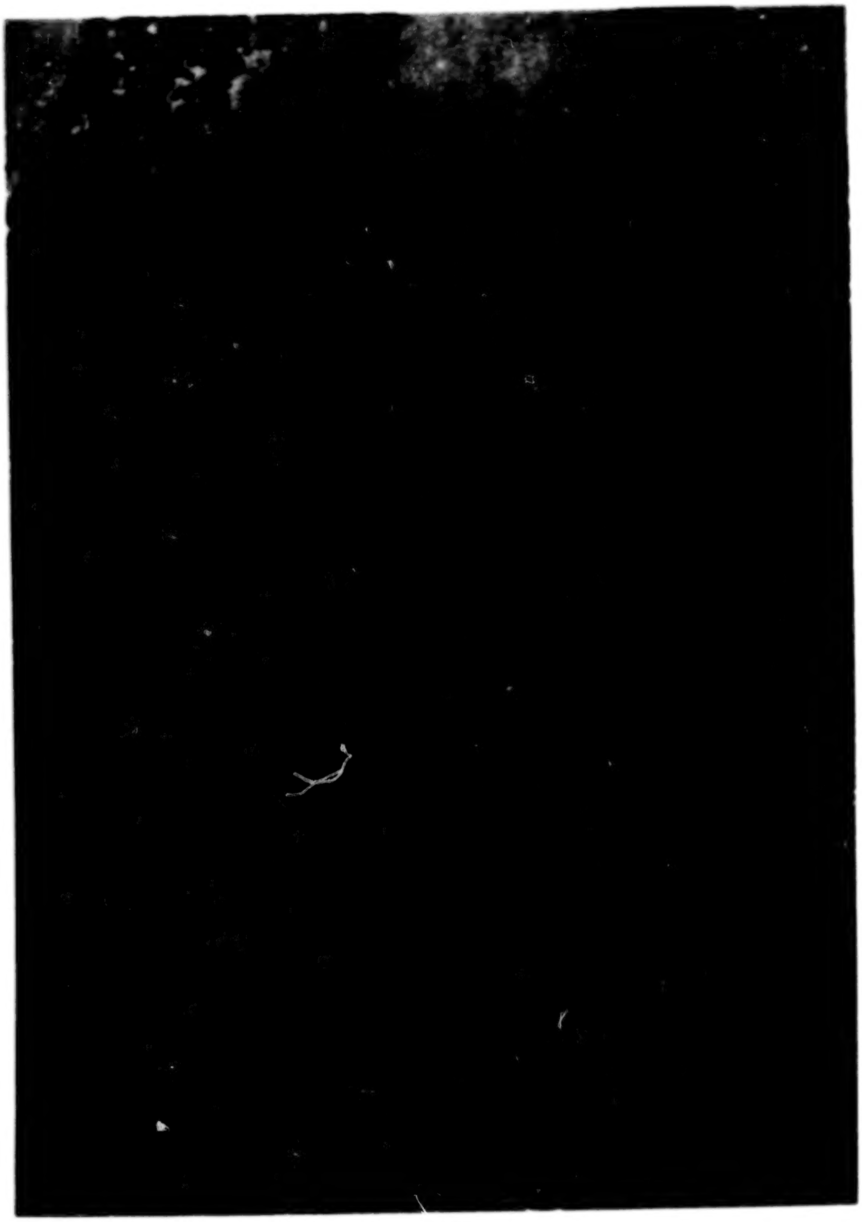

Fig. 2
View of the Rocket "Aviavnito" with the Engine on the Launch Stand,
Before its Flight on August 15, 1937

injector, pendulum gears for control of the on and off cycle, additional electrical and pyrotechnical sources for ignition, and optical systems for recording the output of the sensors, etc.

As a result of this research, engineers established the influence of liquid propellant components on the process of ignition, the intensity of the source of ignition, its location, the degree of atomization and propellant mixture, the relative rate of flow of the propellants into the chamber, etc. Also, the period of ignition delay was established as well as the duration of propellant combustion. The resultant recommendations for starting the nitric acid LPREs provided for the dosage of fuel at the moment of ignition, of the expenditure and mixture ratio of propellant components, and the implementation of ignition and the burning process by controlling propellant flow, made possible in future efforts to increase the reliability of operation of the LPRE.

Researching Combustion and Cooling Processes in the LPRE Unit

This research was conducted on a laboratory model of the nitric acid LPRE unit with a thrust range between 30 and 80 kg and a corresponding pressure in the chamber between 7- and 25 atmospheres. The water-cooled model had sets of interchangeable nozzles, cylindrical parts of the combustion unit, and of centrifuge atomizers for the fuel and oxidizer. Modification of these elements allowed for "modeling" the various cycles of firing and for determining the influence of the different elements on the duration of the firing and its performance characteristics.

The data received from this research provided a clear picture of the phenomena accompanying the firing of nitric acid LPREs, and allowed the formulation of a series of recommendations for future planning. Among the most important were the following propositions:

1. To obtain steady burning and a high completion of combustion, it was necessary to feed the propellants through suitable injectors that assurred the formation of a stable front of flame in a cross-section of the chamber.
2. To obtain reliable external cooling of the combustion chamber and of the nozzle using propellant components, it was necessary to provide variation of the flow in the cooling channels around the surfaces according to the changes in the thermal conditions of the walls of the combustion chamber and the nozzle.

These findings made possible the construction of nitric acid LPREs of a more advanced design, with a corresponding improvement in the reliability of their firing.

THE NITRIC ACID LPRE, MULTIPLE-FIRING "RDA-1-150," FOR THE ROCKET GLIDER "RP-318"

The "RDA-1-150" engine was conceived and developed during 1938-1939 using the basic results of the scientific research described above and the experience accumulated in work on the nitric acid LPRE at RNII earlier. It was designated for use in the rocket glider "RP-318." The design of the "RDA-1-150" engine is presented in Figure 3. Engine performance was characterized by the following data:[8]

1.	Propellants	nitric acid and kerosene
2.	Thrust regulated in the range of	80/150 kg.
3.	Pressure in the chamber	8/18 atms.

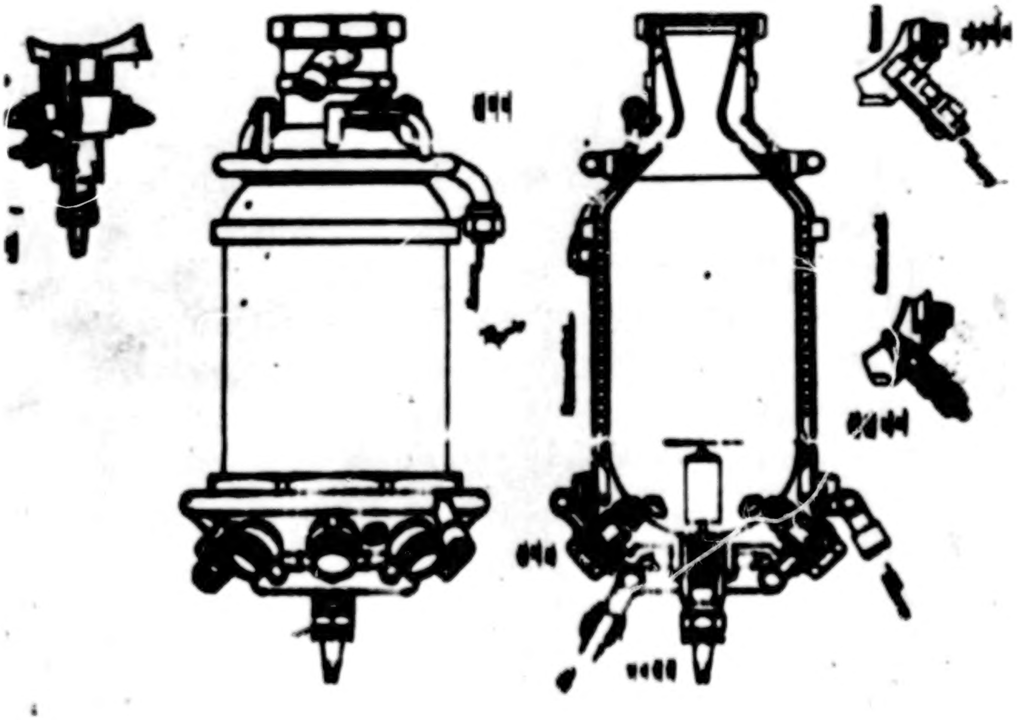

Fig. 3
Schematic of the RDA-1-150 Nitric Acid and Kerosene Engine
for the Rocket Glider "RP-318"

4.	Specific impulse	150/198 sec.
5.	Pressure of fuel feed	13/35 atms.
6.	Diameter of sections of the nozzle	25.5/50 mm.
7.	Volume of the chamber	2.2 L.
8.	Overall dimensions	420 x 200 mm.
9.	Weight of the chamber (or unit)	10.5 kg.
10.	Duration of an uninterrupted firing	200 sec.
11.	Operating lifetime	28 min.

The features of the "RDA-1-150" design that distinguished it from earlier RNII nitric acid LPREs were the following:[9]

1. Mixing of propellants in the combustion chamber by means of injectors.
2. Engine cooling with two propellant components: the nozzle with kerosene and the cylindrical combustion chamber with nitric acid, with their circulation along spiral canals at changing rates of flow,
3. Thermal protection of the injector plate of the chamber by internal cooling,
4. Dual-stage ignition with the application of an electrical spark and propellant flow controls,

5. A modular design composed of three principal components: the head, the chamber, and the nozzle.

During final tests, the engine performed close to the pre-test calculations. The engine demonstrated stability and reliability during burning which guaranteed its admittance to flight testing in the "RP-318" rocket glider. On February 28, 1940, this rocket glider, with test pilot V. P. Fedorov at the controls, was flown successfully.[10] This was the first flight of a Soviet pilot in a rocket-propelled aircraft.

A view of the rocket glider appears in Figure 4. The design of the "RDA-1-150" engine became a prototype for other, more powerful models of nitric acid LPREs that we built later.

ALCOHOL-OXYGEN LPRE, MULTIPLE FIRING "RDK-1-150," WITH A REGULATED THRUST OF 70-160 kg

This engine and its test stand firing procedures were developed in 1939-1940. This alcohol-oxygen LPRE was likewise designated for use in a rocket glider with the goal of compiling tests for comparison of its technical data and qualities with the nitric acid engine. It was therefore calculated that this engine would perform with characteristics similar to those of the nitric acid LPRE "RDA-1-150" in the rocket glider "RP-318". The design schematic of the "RDK-1-150" engine is presented in Figure 5.

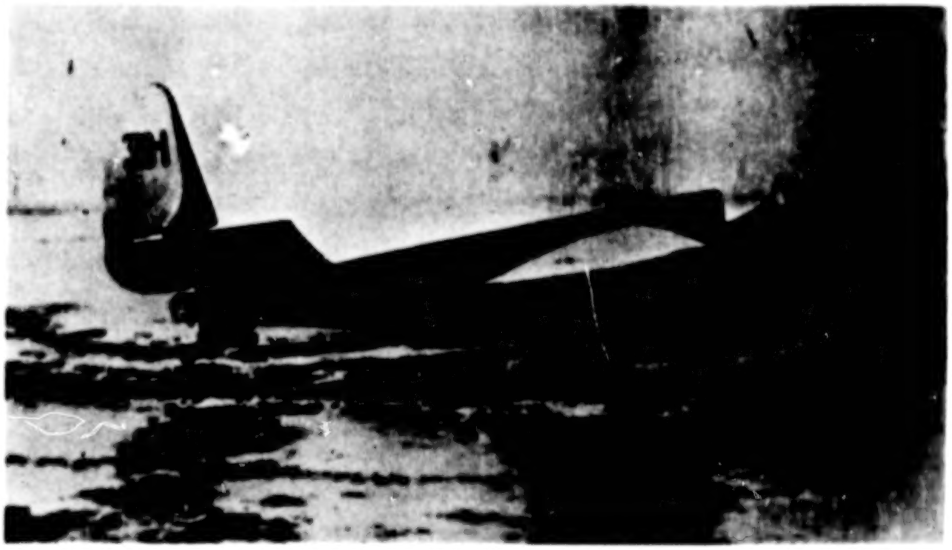

Fig. 4
View of the "RP-318" with the RDA-1-150 Engine Before the First Flight on February 28, 1940

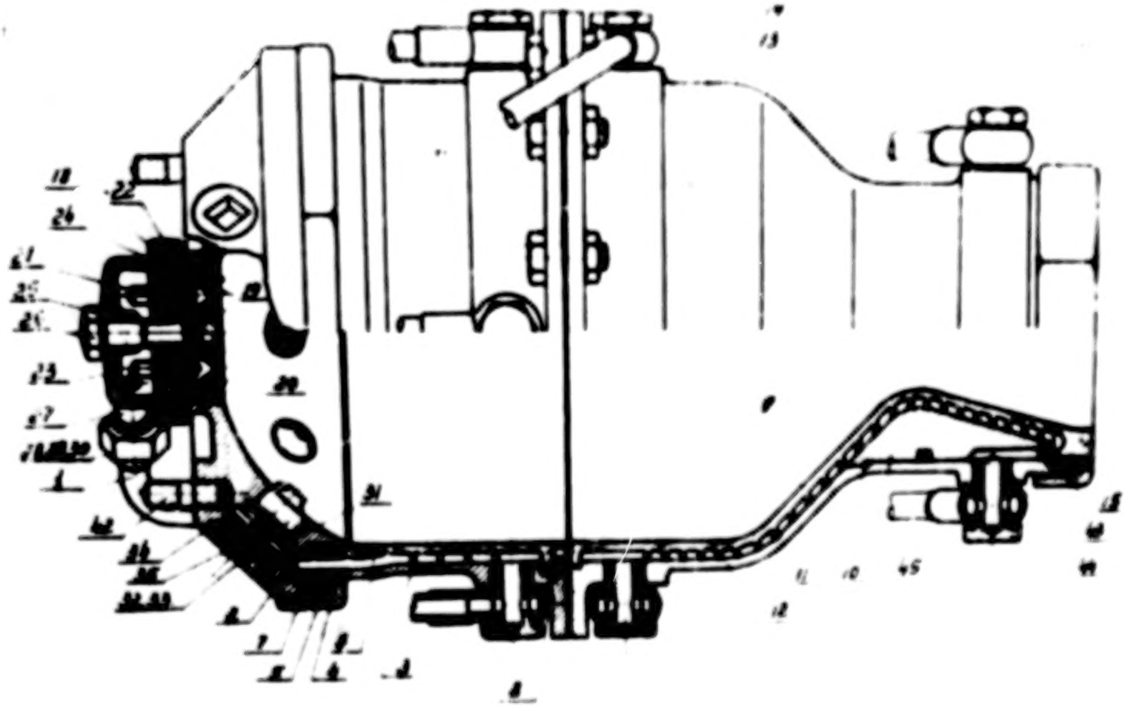

Fig. 5
Schematic of the Alcohol-Kerosene, Multiple-Firing RDK-150 LPRE

This engine featured the same design decisions successfully realized in the "RDA-1-150" nitric acid engine, VIZ., an injection, cylindrical combustion chamber, dual-propellant external cooling--the nozzle with alcohol, the cylindrical combustion chamber with liquid oxygen--and a staged ignition system with electrical spark and modular construction. The following technical data characterized the performance of the "RDK-1-150" engine:[1]

1.	Thrust	70/160 kg.
2.	Pressure in the chamber	5/11 atms.
3.	Specific impulse	150/210 sec..
4.	Pressure of fuel feed	8/20 atms.
5.	Diameter of sections of the nozzle	27.5/64mm.
6.	Volume of the combustion chamber	1.8 L.
7.	Overall dimensions	340 x 220 mm.
8.	Weight	8.5 kg.
9.	Duration of an uninterrupted firing	180 sec.
10.	Operating lifetime	20 min.

Test stand firings produced data close to the pre-test calculations. Ground tests of the "RDK-1-150" engine in a model of the glider "G-14," staged to be redesigned as a rocket glider, were conducted in 1940. Flight tests of the engine proved unsuccessful. The value of the "RDK-1-150" engine development resided in utilizing designs similar to those of the nitric acid LPRE, and showed the possibility and advisability of an oxygen LPRE that could be ignited repeatedly.

A COMBINED SOLID- AND LIQUID-PROPELLANT ROCKET ENGINE, THE "KRD-604," WITH A STEP-UP THRUST OF 1,100-TO-5,000 kg

The "KRD-640" engine was developed in 1939-1940 on the basis of combining in a single design, both solid and liquid-propellants. This combination of two types of engines was accomplished in a manner allowing the engine to be fired at first as a solid propellant engine with a high thrust; after the solid propellant supply in the chamber had burned away, it switched automatically to burning liquid propellants with a lesser thrust, but with an increase in duration.

The first ignition guaranteed a high starting acceleration and, naturally, the accuracy of the flight; the second ignition ensured the length of the flight. Nitroglycerine-based powder, nitric acid and kerosene were employed as propellants, the latter fed into the chamber by means of a solid-reactant gas generator. A device that switched the engine automatically from solid- to liquid-propellants and triggered the gas generator, was the chief design feature. The peculiarity of this design involved unifying within the body of the rocket propellant tanks and a propellant feed system without piping and intermediate compartments.

The schematic of this "KRD-604" engine, and that of the "RDD-203" rocket in which it was utilized, are shown in Figure 6.[13] The "KRD-604" engine produced the following data:[12]

1.	Thrust	4,500/1,030 kg.
2.	Pressure in the chamber	220/48 atms.
3.	Specific impulse	200/220 sec.
4.	Pressure of liquid fuel feed	75 atms.
5.	Diameter of sections of the nozzle	39/132 mm.
6.	Duration of firing	0.7/9 sec.
7.	Total impulse of thrust	12,300 kg./sec.
8.	Volume of combustion chamber	18 L.
9.	Overall dimensions of the chamber	950 x 203 mm.
10.	Weight of the assembly	46 kg.

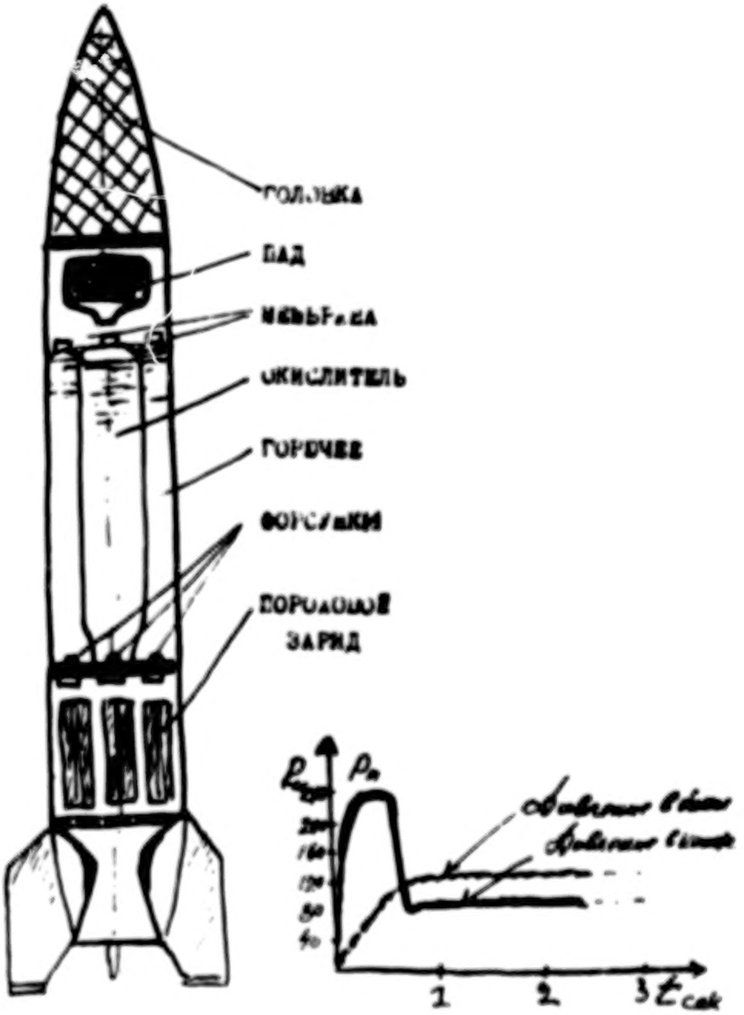
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Fig. 6
Schematic of the Installation and Components in the Rocket with the Combined Solid- and Liquid-Propellant KPD-604 Engine

In 1940-1941, test launches of the combined "KRD-694" engine in the "RDD-203" rocket—which had an initial weight on the order of 180 kg—achieved distances of about 20 km (against a calculated distance of 22.5 km).[14] A view of the "RDD-203" rocket on the launch stand for single and group launches is shown in Figure 7.

THE NITRIC ACID LPRE "D-1-A-1100" WITH A CONTROLLABLE THRUST OF 350-1, 400 kg, FOR ROCKET AIRCRAFT

The "D-1-A-1100" engine was designed in 1941 and assigned as a basic engine for application in rocket aircraft interceptor. Nitric acid and kerosene, fed into the

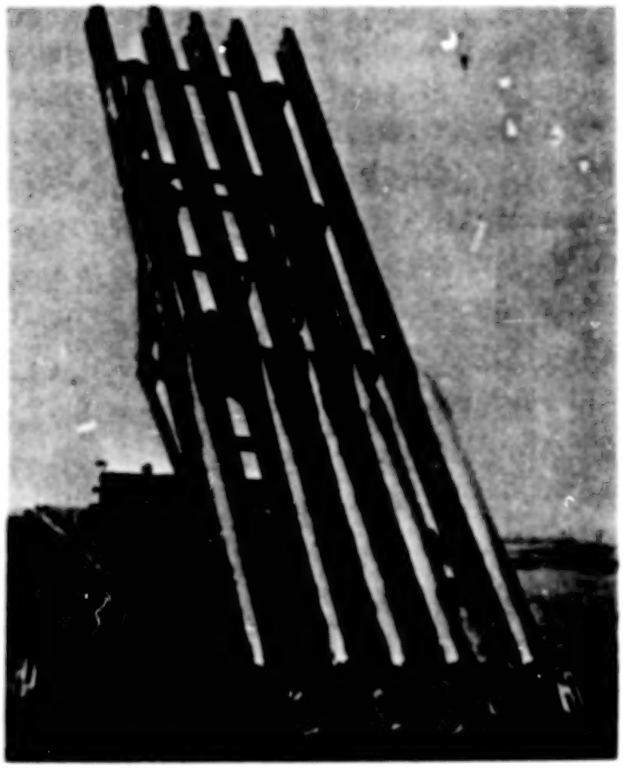

Fig. 7
View of the Mobile Launcher for Multiple Firing of RDD-203 Rockets
With Combined Rocket Engines in Firing Position, September 1940.

chamber by a pressure-fed system, served as propellants. The design schematic of this engine is shown in Figure 8. The engine design basically corresponds to the 150 kg-thrust "RDA-1-150" engine described earlier.

The "D-1-A-1100" engine possessed the following basic technical data:[5]

1.	Thrust	1,400/350 kg.
2.	Pressure in the chamber	19/5.8 atms.
3.	Specific impulse	203/156 sec.
4.	Pressure of fuel feed	48/17 atms.
5.	Diameter of sections of the nozzle	39/132 mm.
6.	Volume of the combustion chamber	19 L.
7.	Overall dimensions	730x310 mm.
8.	Weight of the chamber with valves	48 kg.
9.	Duration of an uninterrupted firing	180 sec.
10.	Operating lifetime	20 min.

During the creation of the "D-1-A-1100", great effort was expended in solving technical and production problems (primarily in connection with the nozzle and its

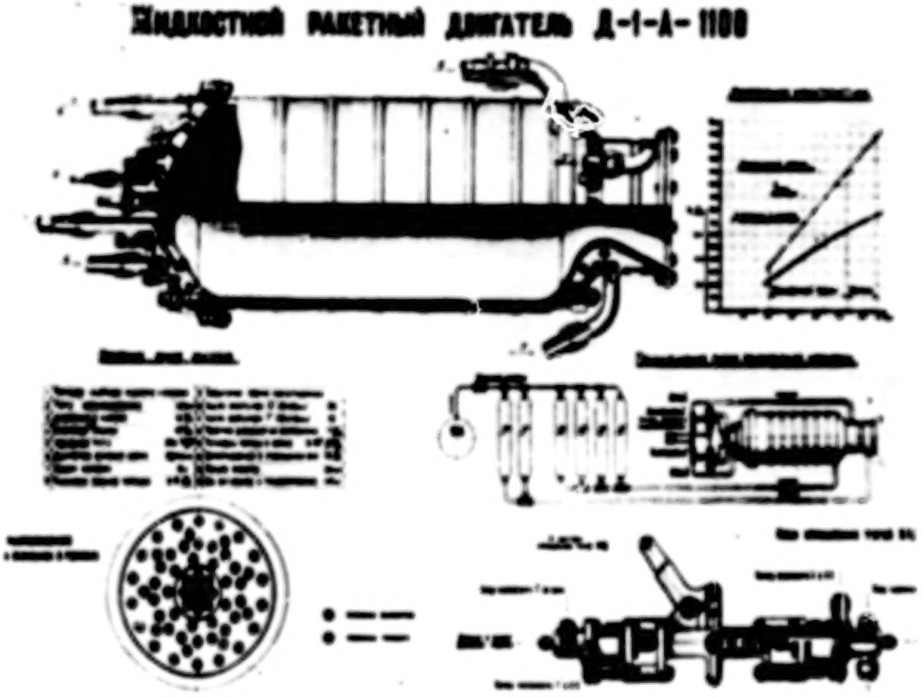

Fig. 8
Schematic of a DI-A-1100 Nitric Acid and
Kerosene LPRE for the Rocket Plane "BI."

cooling) of this "scaled-up" engine that produced ten times more thrust (in a single chamber design) than had its prototype. In the course of the work, research was conducted to establish the true quantities of heat flow and its distribution along the length of the chamber and nozzle. Special metallographic studies based on the evaluation of the critical points during phase transformation of the metals were made of the nozzle after firing the engine.

Testing of this engine was successfully completed in April 1942, and it was ready for installation in the "BI" aircraft, designed under the direction of V. F. Bolkhovitinov. The first test flight took place on May 15, 1942, with G. Ya. Bakhchivandzhi as the pilot, and was recorded as the first Soviet manned flight of a rocket interceptor aircraft. A picture of the "BI" plane before takeoff appears in Figure 9.

Fig. 9
A View of the Rocket Plane "BI-1" and of the D-I-A-1100 Engine Before its First Flight, May 15, 1942.

SELF-CONTAINED AIRCRAFT LPRES OPERATING ON NITRIC ACID AND KEROSENE WITH A PUMP-FED PROPELLANT SYSTEM: THE SINGLE-CHAMBER "RD-2M" AND DUEL-CHAMBER "RD-2M3V"

We began work on the creation of self-contained LPREs with a pump-fed propellant system in 1940, and it became the subject of intensive work after 1942, when the limitation in the "BI" aircraft of a pressure-fed propellant system became apparent. As a result of the extensive design study carried out in 1942-1943 and experimental work performed in 1944, two models of automatic LPREs burning nitric acid and kerosene with a pump-fed propellant system were developed, built, and stand-tested under laboratory conditions: the single-chamber "RD-2M" with a variable thrust in the range of 350-1,400 kg, and the dual-chamber "RD-2M3V" with a thrust of 100-1,500 kg, each identical in construction and design of the basic models, but distinguished from each other by the range of the variable thrust. Both worked according to the same principle. The design of the dual-chamber "RD-2M3V" engine is shown in Figure 10.

Four basic subsystems characterized these engines: the combustion chamber, steam-gas generator, turbine pump unit, and the ignition-regulating subsystem, mutually interconnected by a single pneumatic-hydraulic system which guaranteed their automatic functioning and control of ignition, engine operation, and the cessation of burning by means of controls located in the cabin of the aircraft. The combustion chambers were

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PART IV

THE DEVELOPMENT OF LIQUID- AND SOLID-PROPELLANT ROCKETS, 1880-1945

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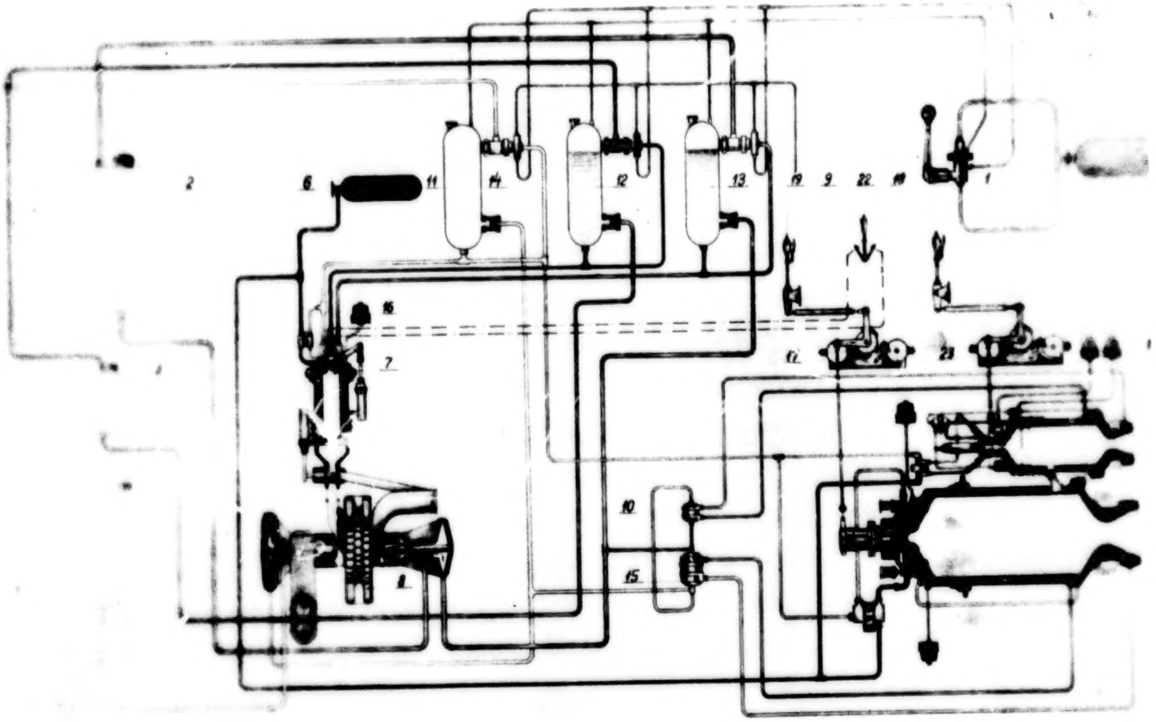

Fig. 10
Schematic of a Double-Chamber Flight Engine AD-2M3B, Using Nitric Acid and Kerosene With a Pump-Fed Propellant System

similar in design to the "D-1-A-1100" engine, but were different in the design of the start-stop propellant control valves for multiple firing in flight.

The vaporizing generator (PGG) worked on the basic propellant components, with water or an alcohol solution injected in the burner for cooling purposes which automatically regulated the temperature of the vapor. The PGG was developed taking into account previous design experience at RNII.

The turbine-pump unit (TNA), developed with the participation of the National Institute of Hydromechanical Construction, consisted of two single-stage centrifugal pumps for feeding fuel and oxidizer into the chamber, and a separate pump for feeding the alcohol solution into the PGG. The supply to the pumps used a two step turbine fed directly from the PGG.

The start-regulating system (PRS) for these engines fulfilled a dual role. First, it served as a device for ensuring multiple firing of the engine, and possessed a microsystem of the pressure-fed system used for starting the [illegible]. Second, it served as a regulating device in the engine to control the thrust, and [illegible] supported the stability of a given pattern and flow return of the propellants into the tanks during periodic shutdowns.

The basic subassemblies of the engines just enumerated and other related components were combined into two modules: the chamber and the pumps, thus permitting their distribution together or separately in different compartments of the aircraft. A photograph of the chamber system of the "RD-2M3V" is shown in Figure 11 and the pumping module in Figure 12.

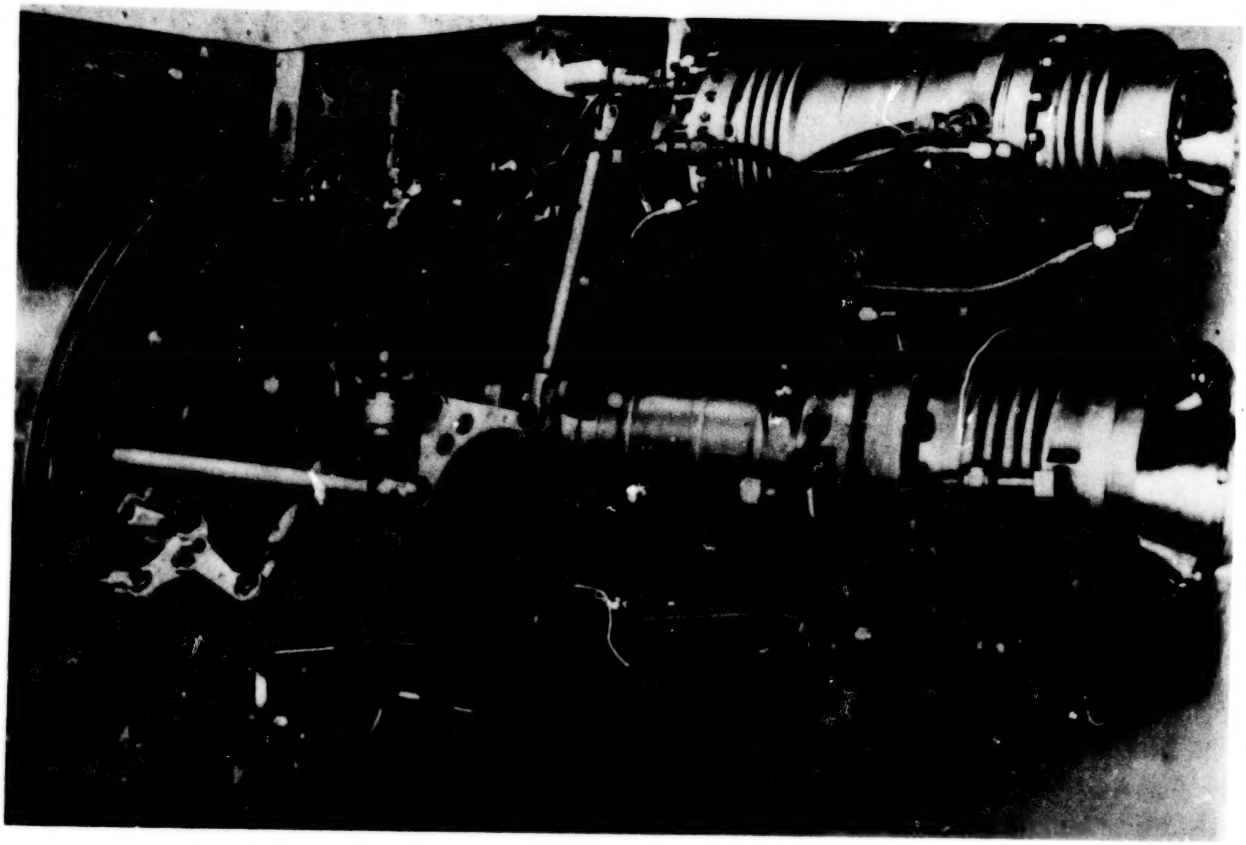

Fig. 11
View of the Chamber Module of the Pump-Fed RD-2M3V Engine

The basic performance data for these engines appears below:

		"RD-2M"	"RD-2M3V"
1.	Thrust	1,450/400 kg.	1,500/100kg.
2.	Pressure in the chamber	20.5/6.7 atms.	20/6 atms.
3.	Specific impulse	200/142 sec.	200/145 sec.
4.	Pressure of feed	44/10 atms.	35/10 atms.

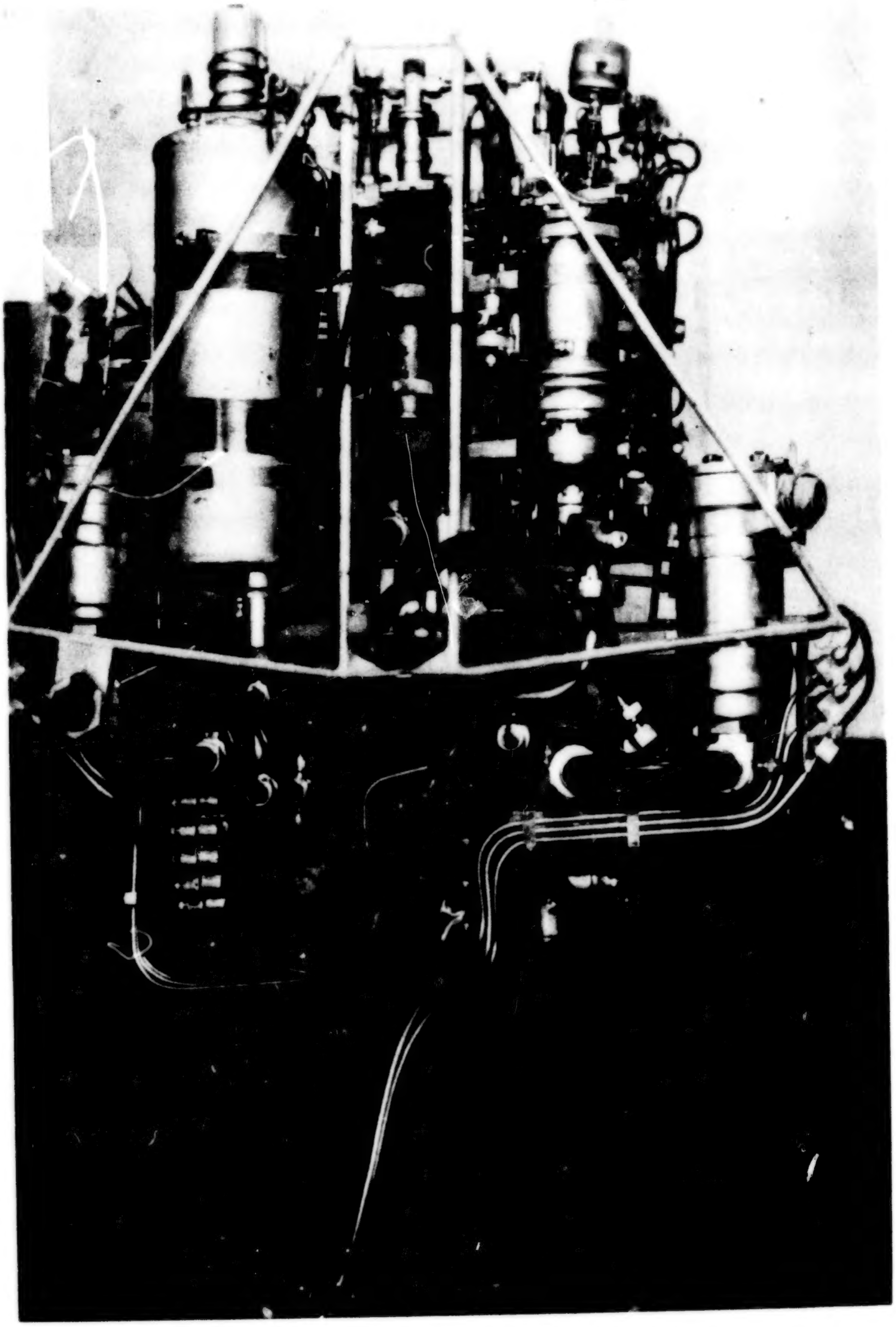

Fig. 12.
View of the Pumping Module of the Pump-Fed RD-2M3V Engine

		"RD-2M"	"RD-2M3V"
5.	Diameter of sections of the nozzle	80/146 mm.	80/146; 35/90 mm.
6.	Volume of the chamber	18 L.	18/4.5 L.
7.	Overall dimensions of the chamber block	925x225 mm.	925x540x220 mm.
8.	Weight assembled	200 kg.	223 kg.
9.	Turbine power and RPM	75 HP/12,500rpm	80 HP./12,000rpm.
10.	Operating lifetime	40 min.	1 hr.

The "RD-2M" and the "RD-2M3V" engines were successfully tested from January through April 1945 in test-stand experiments under government control and provided revealing data; they became the first models of Soviet automatic aircraft LPREs that featured pump-fed propellant systems with multiple start. During tests in flight and when assembled in the experimental rocket aircraft "I-270", the engines proved reliable. A photograph of the "I-270" is shown in Figure 13.

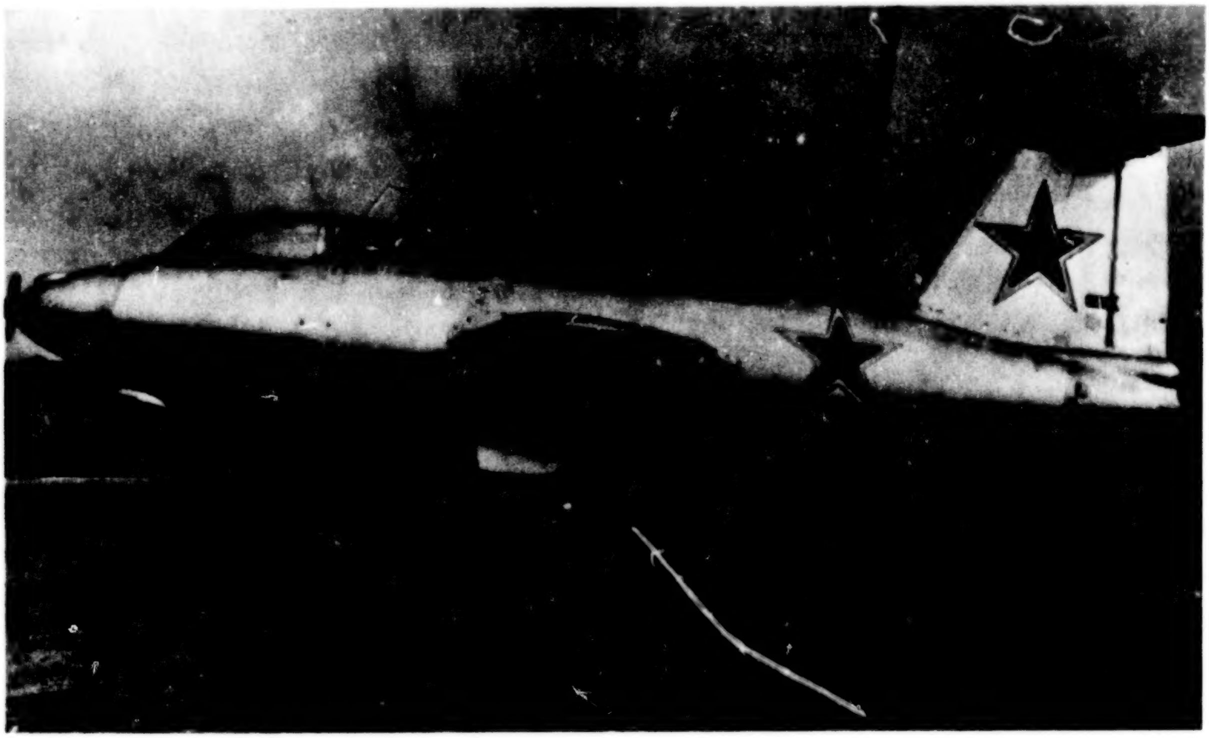

Fig. 13
A View of the Rocket Plane With the RD-2M3V Engine
Before its First Flight in September 1947.

In concluding my paper I think it is necessary to stress that this survey of LPREs is only a small part of the scientific and technical work undertaken in this field in the USSR between 1934-1944. Moreover, these experiments demonstrated the independent and self-reliant paths adopted in the Soviet Union for mastering the LPRE and for introducing new technology, as well as the originality in solving the major scientific and technical problems connected with LPRE designs of different types and requirements.

We recognize the importance of F. A. Tsander's role in this work, but it is impossible not to mention the exceptionally fruitful, collective work of many engineers, designers, and experimenters, a large amount of whom were followers of his school. Their energy, persistence, and capacity were shown at different times in achieving positive results during the testing and design development in the field of LPRE.

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ANALYSIS OF LIQUID-PROPELLANT ROCKET ENGINES DESIGNED BY F. A. TSANDER[+]

Leonid S. Dushkin and Yevgeny K. Moshkin (USSR)[++]

One of the characteristics of the scientific and technical activity of F. A. Tsander in the area of the development of rocket technology and astronautics was his great activity and productivity in formulating and conducting engineering work on the practical utilization of the principles of reactive motion. He was moved by a deep conviction of the possibility of accomplishing manned space flight in the immediate future within the existing level of science and technology. A prominent scientist in researching a wide circle of problems in the theory of reactive motion and interplanetary flight, Tsander devoted a significant part of his efforts to practical engineering and design problems directed toward producing experimental liquid-propellent rocket engines suitable for use in flying vehicles of various types.

An extensive list of topics for practical investigation and development, formulated by Tsander, was devoted mainly to the energetics of reaction engine power plants. It included investigations in the application of metals and supplementary fuel in liquid-propellent rocket engines (LPRE), the successful application of new thermodynamic cycles, the development of designs and utilization of operating processes in LPRE, and the search for ways to increase the efficiencies of the assembly and the specific impulse of the engine. First among these tasks was the problem of producing reliable samples of LPRE. Tsander gave great consideration to the practical development of LPREs, and considered it basic to the successful development of rocket technology and the creation of the foundations of astronautics. At the same time he clearly recognized the great complexity and difficulties in solving this problem.

The high scientific and engineering erudition of Tsander in the area of aviation engine design and related technologies, his insight into choices for developing rocket

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[+] Presented at the Sixth History Symposium of the International Academy of Astronautics, Vienna, Austria, October 1972.

[++] Dushkin specialized in rocket engine design and development during the 1930s and 1940s in GIRD, RNII, and other scientific institutes. Moshkin, a specialist in liquid rocket engine injector design, is an engineer presently affiliated with the Ministry of Aviation Industry.

technology, and his knowledge of the state and level of developments in various areas of science in the USSR and abroad, permitted him to develop his own original program of work—a program designed for successful completion with a minimum of effort and expense, and within the limited possibilities of obtaining material and technological assistance. This program provided for the development of methods for engineering calculation of engine processes and the choice of the optimum parameters for the operation of its assemblies; the development of laboratory models of LPRE and experimental tests to verify theoretical calculations, and the selection of rational shapes and dimensions of the structural elements; the construction and finishing of engines, mounting them in test vehicles, and carrying out flight tests.

F. A. Tsander successfully realized this program between 1928 and 1933. He performed the first and second stages in individual order without interrupting his main activity in the aviation industry. He performed the third stage of work with a small group of young specialists in the experimental-design organization, the Group for Study of Jet Propulsion (GIRD), created in Moscow during the fall of 1931 and headed by S.P. Korolev.

A number of studies in the history of the development of rocket technology and astronautics consider the practical works of F.A. Tsander on LPRE as important stages in the development of Soviet rocket technology, but his works are often dealt with schematically, without a detailed analysis of their directionality, originality, and maturity of creative solutions, or their scientific, technical, and practical value. An attempt is made in this report to deal with the practical works of Tsander on LPRE in more detail, on the basis of preserved archive materials, and also on the basis of personal recollections of the authors of the report, since they were fortunate to be his students and work under his direction in GIRD on LPRE during 1932-1933.

The development of methods for calculating the engineering performance of LPREs began to occupy Tsander at the beginning of the 1920s. His basic idea involved the construction of an interplanetary craft combining the features of an airplane with a liquid-propellent rocket, in which certain metallic parts of the airplane and rocket structure are used as supplementary fuel after they are no longer needed, when the craft had passed out of the atmosphere.

The choice of propellents was a fundamental problem. From the very beginning, Tsander dwelt on his choice of cryogenic propellents as having the highest energy potential--liquid oxygen as the oxidizer and liquid hydrogen as the fuel. Later he regarded liquid hydrogen as promising the greatest potential, but for the initial stage of the work he chose the more readily available aviation gasoline as the fuel for LPRE. The orientation toward aviation gasoline as the fuel in an oxygen LPRE, instead of liquid hydrogen, resulted not only from the difficulties in producing and handling liquid hydrogen during this time, but also because Tsander, in developing his ideas about a combined airplane and rocket in an interplanetary craft, wanted to use a single fuel for both engines onboard the craft.[+]

F. A. Tsander provided for the possibility of using liquid oxygen not only in pure form, but also in the form of a mixture of liquid air with various values of oxygen concentration, that permitted not only performing work in stages on the LPRE, but also improved the performance of the ordinary aviation piston engines.

An important theoretical result of Tsander's work was the development of an engineering method for propellant calculations of oxygen-propellent rocket engines that, for practical purposes, determined the basic parameters of the engine and the structural elements. Consideration of the effect of dissociation of gases, precise determination of the heat transfer coefficients of the gases to the chamber wall and of the wall to the surrounding medium, and the calculation of the volume and shape of the combustion chamber, were characteristics of this method. This method produced data close to the actual values, and established Tsanders calculations in LPRE in the class of mature and original scientific and technical solutions. These calculations were performed during 1930-1931, but were first published only in 1937 in the form of an article in the collection *Raketnaya tekhnika* (Rocket Technology) No. 5, under the title "Thermal Calculation of a Liquid-Propellent Rocket Engine."

EXPERIMENTAL INVESTIGATIONS OF THE OPERATING PARAMETERS AND STRUCTURAL ELEMENTS OF A REACTION ENGINE ON THE LABORATORY MODEL

Occupied with the development of methods for engineering calculation of the operating processes and structural elements of LPRE, Tsander became convinced that it was impossible to give a satisfactory answer to many problems without obtaining experimental data lacking in the related sciences and technology. This involved propellent mixture formation in the chamber, combustion processes, discharge, heat exchange, etc. Thus arose the necessity of producing a laboratory model to experimentally verify the initial assumptions. The production of LPRE models thus assumed great importance in formulating and mastering experimental techniques.

The result was the production of the reaction engine OR-1. Without dwelling on a description of the OR-1 device, which is covered in the technical literature, we will note only the characteristics and role of this engine in design developments on LPRE. Tsander prepared calculations for the OR-1 according to his original technique for

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[+] In the 1930s Eugen Sänger pursued a similar line of research in Germany, as he sought to create a "space transporter." See Irene Sänger-Bredt, "The Silver Bird Story: A Memoir," in this volume--Ed.

determining the parameters of LPRE. Liquid oxygen in mixture with liquid air served as the oxidizer. It is natural that the first tests of the engine began with the simplest version, i.e., with ordinary compressed air, with consideration of its gradual enrichment with oxygen, concluding with pure oxygen.

Experimental results obtained in tests of the OR-1, carried out with gasoline and compressed air, verified the calculation techniques, and gave Tsander the basis for continuing to design LPRE with the use of liquid oxygen and gasoline as propellants. The OR-1 engine permitted selecting the design solutions for the LPRE chamber. In this respect, the OR-1 should be considered a prototype of the OR-2 oxygen LPRE also produced by Tsander.

In evaluating this stage of Tsander's experimental work on the OR-1 laboratory model, one can conclude that it marked the production in the USSR of the first operating model of a rocket engine operating with liquid fuel and a gaseous oxidizer, and significantly aided the systematic development of works in producing oxygen LPRE and rocket vehicles at GIRD.

DEVELOPMENT OF THE GASOLINE-OXYGEN LPRE OR-2 FOR THE PILOTED VEHICLE (ROCKET PLANE) RP-1

One of the problems posed by this new technology in the USSR was the development of a suitable aviation LPRE. In order to provide as high a specific impulse as possible, and taking into account the promise of cryogenic fuels, Tsander selected liquid oxygen and gasoline as propellants for the OR-2 engine. In the long-range plan of LPRE development, he still contemplated using metal to supplement the propellants, and also proposed using the so-called "converging and diverging cone," in which the thermal action and acceleration of the gas flow was accomplished. However, taking into account the necessity of developing the OR-2 engine in a short time and providing for its high operational quality, Tsander refrained from applying metal additives and the "converging and diverging cone" at this first stage of work.

On the basis of preliminary calculations, he selected 300 sec as the engine operation time, sufficient for valid testing of the engine in flight, a thrust of 50 kg, sufficient for accomplishing flight, and a chamber pressure of 6-8 kg/cm^2. In such a pressure regime with a sufficiently high specific impulse, the heat flow from the gases to the chamber wall, as determined by Tsander's calculations, would have relatively small values, and reliable cooling of the combustion chamber could be successfully provided.

It is important to note that Tsander accomplished the development of LPRE in the first stage of work. The complex engine plant included all the systems and devices necessary for operating the LPRE in flight on the piloted vehicle--the combustion chamber, the fuel supply system, the launch assembly, and the ignition, operating mode, and cut-off controls. We will also note briefly some of the structural characteristics of the developed OR-2 engine. Tsander gave great consideration to the solution of the problems of propellant mixing. Taking into account the properties of gasoline combustion, he designed the structure of the chamber so that individual jets of gasoline, delivered to the chamber toward the nozzle along the chamber axis, were broken up into fine particles by the gaseous oxygen entering the OR-2 combustion chamber at its upper part radially through slots in the chamber wall. The fine drops of gasoline mixed well and burned with the proper completeness in the medium of gaseous oxygen.

Tsander considered the creation of a reliable chamber and nozzle cooling system as an important problem in producing LPRE. His calculations showed that liquid oxygen had better characteristics than gasoline as a cooling liquid. Moreover, the liquid oxygen evaporates in cooling the chamber walls, improving the mixing, which was just discussed.

Thrust regulation was first provided in the design of the OR-2 engine. The "nozzle valve" was developed for this purpose. Depending on the position of the control lever, various numbers of radially positioned holes were gradually covered in the inner cavity of the valve. Each fixed position of the control lever corresponded to a specified thrust developed by the engine. In designing the engine, the possibility of multiple firing of the engine was considered and provided for with the application of a high-voltage spark plug as the ignition source.

Tsander also accounted for the technological possibilities of production; as a result, the entire engine assembly was fabricated successfully and made ready for tests. The design of the OR-2 allowed for disassembly, and individual components and parts could be replaced or repaired during final adjustment and operation. Tsander and his students developed a flight model that assisted placement of the assemblies of the engine plant in the RP-1 rocket plane.

The firing tests of the OR-2 were carried out without F. A. Tsander, who was in the hospital in Kislovodsk during the test days. The tests occurred on March 18, March 21, March 26, and April 28, 1933. These were the tests of the first engine installation of LPRE produced at GIRD in a form suitable for mounting in a flying vehicle. Of course, the first tests did not achieve the expected satisfactory results. There were chamber burnouts, explosive combustion processes with firing, etc. Nevertheless, the results of the experimental data verified the possibility of implementing LPRE for piloted vehicles with high-caloric fuel. To increase the reliability of operation of the engine, improve its operational qualities, and reduce the time for preparing the engine for test, it was necessary to carry out laborious final adjustments and search for and select the most rational solutions with partial improvement of the structural elements of the engine.

This improvement, accomplished by Tsander's students A. I. Polyarny, the authors of this report, and other colleagues at GIRD, mainly involved realizing the following measures. First, to decrease the temperature of the gases in the chamber, it was decided to replace the gasoline with a highly concentrated solution of ethyl alcohol, which had been specified by Tsander in case difficulties were encountered with gasoline. Second, to improve the heat-shielding of the combustion chamber and nozzle, it was decided to protect the inner walls of the chamber and nozzle with a refractory heat-insulation layer. Coatings based on magnesium oxide were used for the nozzle, and aluminum oxide for the chamber. This permitted simplifying the design of the combustion chamber and fuel supply system.

The new version of the engine was identified as 02, and was finally prepared at the Jet Propulsion Research Institute (RNII) during 1934-1935. The firing tests, operating with liquid oxygen and 92% ethyl alcohol, obtained a combustion chamber pressure of 11 kg/cm^2, a thrust of 100 kg, a specific impulse on the order of 200 sec, and a firing duration of 60 sec. In 1936 the 02 engine was applied in flight tests of the 206 winged rocket designed by S. P. Korolev. Installation of the original OR-2 engine on the RP-1 rocket plane was not accomplished because the airframe was used for other purposes as the light airplane BICh-1.

DEVELOPMENT OF THE 10 ENGINE FOR THE GIRD-10 ROCKET

The second engine developed by Tsander was intended for the unguided liquid-propellent rocket GIRD-10, produced in 1932-1933 at GIRD for solving a number of scientific research problems. It differed from the GIRD-09 rocket developed during this same period in that both propellant components were stored in the GIRD-10 rocket in liquid form. Thus, the GIRD-10 rocket was the first domestic rocket that operated with LPRE. Numerous calculations carried out by Tsander, and particularly his students, preceded the design of this engine. In the initial version of the 10 engine, it was proposed to use liquid oxygen, gasoline, and molten or powdered metal as a fuel additive. However, in anticipation of technical and technological difficulties, this version was rejected.

After further calculation and design searches, the development of LPRE for the GIRD-10 rocket was accomplished with the application of liquid oxygen and ethyl alcohol. This engine, given the index 0-10, as well as the OR-2 engine, was initially designed by Tsander in the form of a complex engine plant coordinated with the parameters and features of the rocket itself. Tsander later participated in its development. The design of the entire engine plant was preceded by thorough hydraulic, gas-dynamic, and structural calculations, with consideration given to the production possibilities available at GIRD. We will briefly note some properties of this LPRE design.

The combustion chamber was all metal and pear-shaped, with a precombustion chamber and external liquid oxygen flow cooling. The absence of heat-insulation refractory coatings permitted producing a chamber of very low weight, which was very important since the chamber was intended for testing on a rocket under flight conditions. The combination of external liquid oxygen flow cooling with its partial evaporation in the cooling channel, and the presence of the precombustion chamber, ensured good mixing of the propellants and complete combustion in the chamber. In this case, the solution of this set of problems can be considered identical in the 0-10 and OR-2 engines.

To sum up the test operation of the 0-10 engine carried out in 1933 without Tsander, the following data were obtained: propellants--liquid oxygen and 85% ethyl alcohol, chamber pressure--10kg/cm^2, thrust--65-70 kg, specific impulse--175 sec. The duration of continuous operation was 15-20 sec. Ignition of the engine was accomplished with a spark plug. An expulsion system from a compressed air accumulator fed propellants to the engine combustion chamber. The mounting selected for the engine plant in the rocket was so successful that it obtained practical application in later designs, and its basic features are preserved to the present day.

The first test flight of the GIRD-10 rocket occurred on November 25, 1933, near Moscow. The entire engine assembly functioned normally during this test; the engine developed a thrust of 70 kg, and the rocket was successfully launched. During the flight, however, because of mechanical malfunctions in the combustion chamber, the rocket changed its direction of flight and landed with the engine still running.

The flight test of the first Soviet LPRE verified the promise of the scientific and technical direction developed at GIRD, and aided in accelerating the development of subsequent rocket designs in the USSR. Fridrikh Arturovich has characterized F. A. Tsander in the area of LPRE as a prominent theoretical scientist and talented engineer, distinguished by exceptional energy and single-mindedness in the realization of his ideas. His calculations, design, and experimental work on LPRE and rockets at the initial stage of development of rocket technology in the USSR show that he created his own school in the theory and design of reaction engines. Besides his engineering and theoretical developments, Tsander fashioned from a number of his students a large group of specialists who worked successfully in the following years in the area of rocket design in industry, in scientific laboratories, and in educational institutions.

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EARLY POSTAL ROCKETS IN AUSTRIA: A MEMOIR[+]

Friedrich Schmiedl (Austria)[++]

My interest in space research and rocketry dates back almost to its active beginning. Some impressions, way back in my childhood, shaped my thinking and became the motivating force for my later experiments. I shall therefore attempt to reminisce about some early developments that established my contact with space research. I was born in Schwertberg, Austria, on May 14, 1902, and the first event that made a permanent impression on my mind occurred in 1910, when space forced itself directly to man's attention. The Earth had to pass through the tail of the Halley comet. The fact that cyan and carbon monoxide bands had revealed in its spectrum brought about deathly fears, and men were not sure whether they faced disaster. This event marked a new phase in human thought after it became evident that space was not that hostile; one could dare to explore it. The event became the motivating force for my later rocket experiments.

There was still another deciding factor that led me toward rocketry. The mail connections to the surrounded Fort Przemysl broke down in 1915 during World War I. A close relative had died in action, and we had no news from some friends there for some time. The paper balloons failed to reach their destination. I was still in high school when I suggested to my professor that we propose the delivery of mail by means of rockets to the Department of War. Unfortunately, the War Department did not take up this suggestion. After the war, on September 8, 1919, I made a completely new experiment whose significance I recognized only some years later. I developed a group rocket from which another rocket was fired, similar to the now commonly used step rockets. Thus I could reach great heights and cover long distances with relatively low expenses.

In 1923 I made the acquaintenance of Prof. Victor Hess who had discovered cosmic rays in 1914 and received the Nobel prize later for this discovery. Impressed by Professor Hess's lectures, I started a series of investigations on the influence of intense short wave radiation on cell plasma (plant buds) at the University of Graz, in

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[+] Presented at the Fourth History Symposium of the International Academy of Astronautics, Constance, German Federal Republic, October, 1970.

[++] An early experimenter with solid-propellant rockets and rocket mail delivery systems in the 1920s and 1930s.

addition to my studies at the Institute of Technology. Although this type of investigation would be important for future space adventures, I abandoned research in this field after I realized that the bacteria and bacilli would go through mutations, and that I might not be able to control their multiplication. So I burned all the radiated plants.

In the 1920s I started some preliminary rocket experiments towards space flight. But first of all I had to convince my professors, who considered my ideas on space flight as a scientific illusion because of my youthful eagerness to assume that space flight was possible. My experiments were carried out in miniature scale. I developed a "micro start tower" which permitted me to test the over-all efficiency of rocket nozzles at various pressures; under the hood of a vacuum pump I fired tiny rockets and tested their efficiency while the air was evacuated. This experimental set-up was not further used in space research, but it was decisive for convincing some of my professors that the efficiency indeed increased with lower pressures.

Unfortunately the vacuum pump at my disposal did not work fast enough. I decided, therefore, to fire a multistep rocket with highly explosive fuel from a balloon at a height of 15,000 meters and test its space suitability right there. The balloon FS 1 was the transport of the small rocket, but it had also to perform some scientific experiments which I could financially afford. The balloon was furnished with magnetized steel wires to hold it in a predetermined east-west position which could be accomplished due to the minimal surface friction at these heights. The rocket was fired in the direction of the Earth's revolution: eastwards. Furthermore, the steel wires had to hold an aluminum flag (300 cm x 70 cm) in a certain position relative to the Sun so that it could reflect the Sun's rays to an observation post on Earth. Thus one could pursue the position of the balloon despite its height. The rocket was also furnished with an aluminum foil top for Sun reflection.

Unfortunately nobody seemed interested in my stratoballoon, and no observatory entered into this experiment. Anyhow, the balloon was found in Hungary and was sent back to me. I should add that my stratoballoon carried some silveracide which would explode at a high altitude. Due to the low density of the air at 15,000 m altitude, visible clouds of ionized dust should eventually form, and the dispersed matter could be moved out of the Earth's gravitational field by solar light pressure. I chose silveracide for my experiments in space since I had tested it before in ground explosion experiments: a grain of silveracid on a steel sheet brought about a strong explosion because the steel proves less tense than the layer of air on top. The brisance makes the air more rigid than the metal and thus a feather will balance in the air above the exploding silveracid without moving, while the steel sheet is deformed. In silveracide explosions with iron present, finely dispersed matter can be detected (similar to the blackening of the inner wall of long used electric bulbs) which is sensitive to light pressure.

My aim was always the construction of space rockets, but I realized of course that a single person would never have the financial means to accomplish it. I hoped that

I might find someone to support the construction of postal rockets with automatic controls, but my hopes were disappointed. In July 1928, I started alone on the solid-propellant postal rockets V-1 and V-2. They were automatically controlled, landed with parachutes, and carried letters with the remark "The final aims of my experiments are postal rockets and space flight." After I had saved some more money to finance further experiments, I constructed V-3 and V-4 in March 1930, and V-5 and V-6 in May 1930. V-5 carried letters where I stated". . . it is theoretically possible to deliver mail from Europe to America via rockets within 40 minutes . . . it is theoretically possible to reach any point on Earth in less than one hour with rockets . . . it is theoretically possible to leave Earth with rockets: space flight." But V-6 failed and the letters were partly burned (Figures 1 through 3).

For experimental purposes I built relatively small rockets about 30 cm long, first of all because I could build them more uniformly, furthermore there was no danger of explosion and they didn't need a parachute for landing. The opening of the parachute (at the right moment) housed in the top of the rocket always proved the most complicated detail in these experiments, and I never solved this problem to my complete satisfaction.

In those years I gained experience that was of advantage at the first public postal rocket flight (V-7) on February 2, 1931, from the Schoeckl Mountain to Radegund. This flight was extraordinarily successful; 102 pieces of mail were delivered and I proved that this new system held out prospect for a fast and reliable postal delivery, especially to isolated mountain areas. But economics forced me to use some rockets twice, adding new fuel cores. Repairing the damage to the casings, however, finally proved to be more costly in time and money than a complete new construction.

In April 1931 I launched three sounding rockets with home-made recording equipment: a spectrograph with Zeiss prisms, and instruments to record the pressure, height, and vibrations. Unfortunately the high initial acceleration and pressure during launch damaged the instrumentation. One rocket was constructed like a Greek column with parallel grooves along the longitudinal axis that had been worked into the aluminum casing to prevent rotation during the flight; this should have permitted an undisturbed spectrum recording. The second rocket, on the other hand, I provided with diagonal grooves in its casing for fast rotation. My purpose was to improve guidance accuracy (like in a spinning centrifugal top) and, furthermore, the rotation could aid in simulating gravitation, at least to a small extent, a feature that might benefit manned space flights. Both models of the sounding rocket were constructed with intermittent thin layers of non-inflammable material between the solid fuel and casing in order to accomplish an undisturbed and shockfree operation of the measuring equipment.

Late in the year, on September 9, 1931, I launched postal rocket R-1. It carried 333 pieces of mail, including 33 special delivery letters. With this enormous

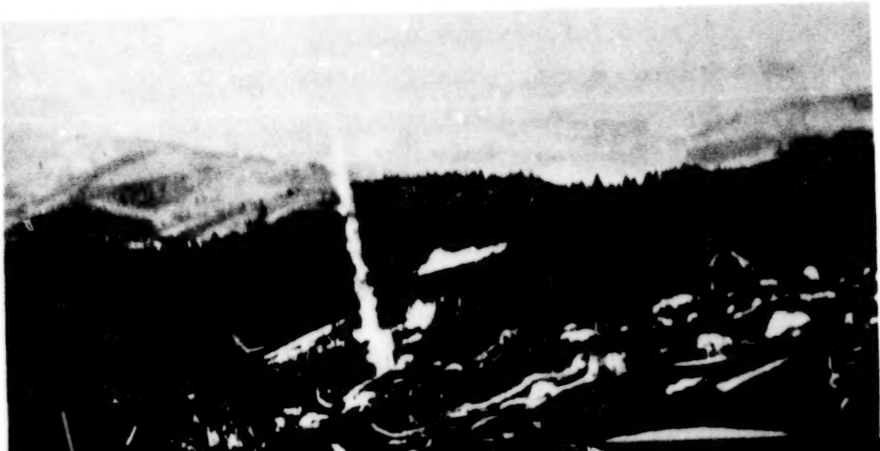
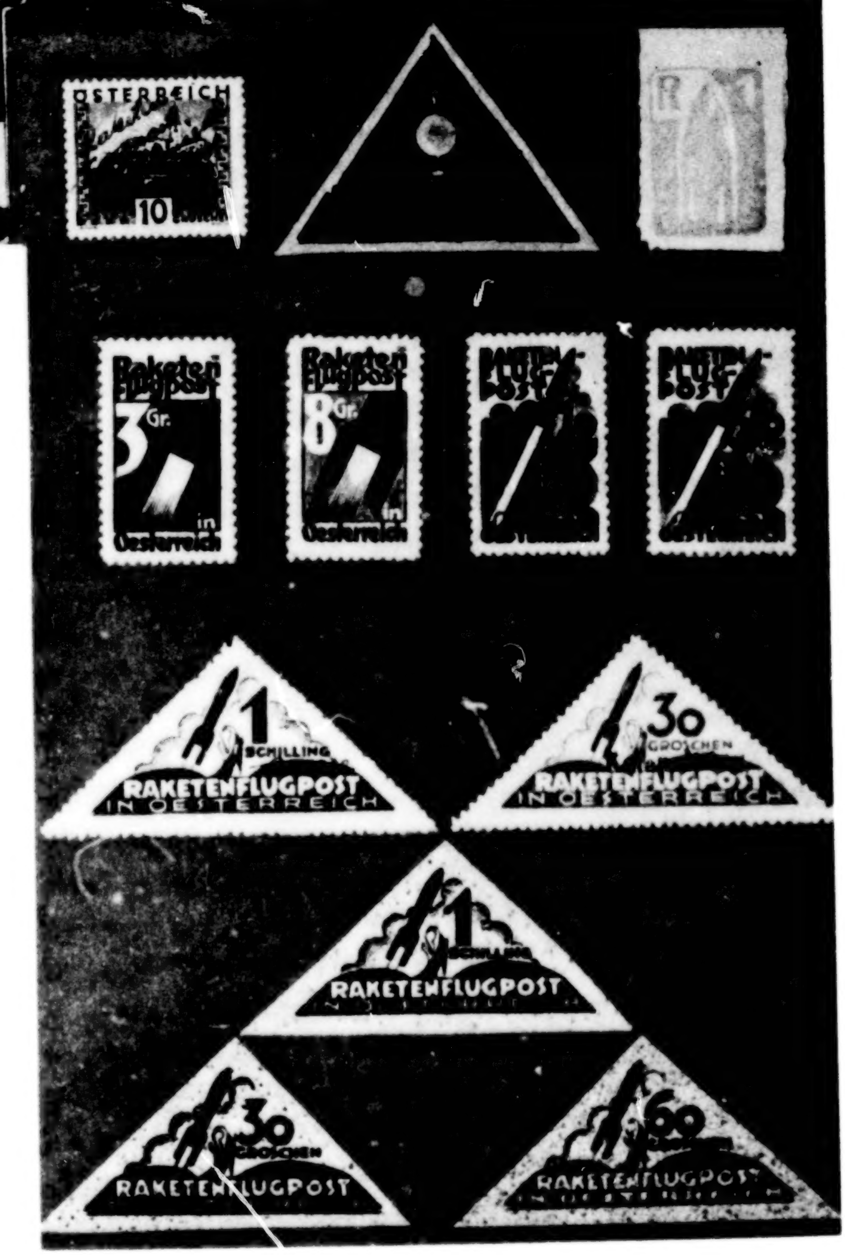


Fig. 1
Mail Rocket Launch,
1928


OSTERREICH
10
Raketen Flugpost
3 Gr.
in
Oesterreich
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8 Gr.
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1 SCHILLING
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IN OESTERREICH
30 GROSCHEN
RAKETENFLUGPOST
IN OESTERREICH
1 SCHILLING
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RAKETENFLUGPOST
60
RAKETENFLUGPOST

Fig. 2
Postal Stamps Used for the
Rocket Mail

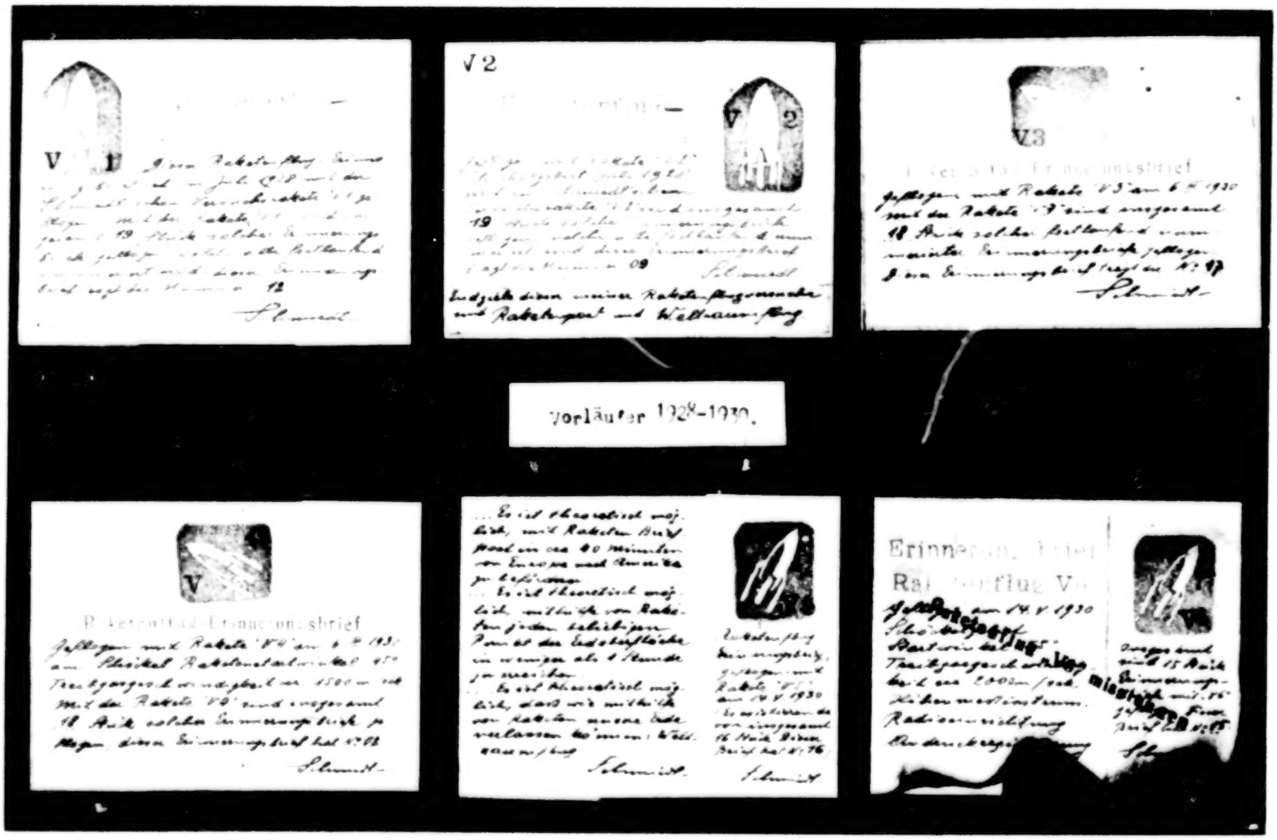
V 1
V 2
V3
V
Vorläufer 1928-1930

Fig. 3
Sample of Mail Posted Between 1928-1930

Moreover, I tried to draw public attention to my experiments and locate someone to support my plans for future research: the construction of a rocket capable of crossing the English Channel (I started preliminary tests in 1932) and, as for the dream of my life, the construction of a space rocket. Such efforts were way beyond my personal financial means, requiring at least three wagon loads of solid fuel which, I am still convinced, is best suited for space.

However, my endeavours to stimulate world-wide interest in space flight were ineffective. I submitted articles and rocket stamps to the major world newspapers, but only one of them bothered to answer--and then with an apology that the subject was not suitable for its readers. To my knowledge, there was never one single word printed, although The New York *Times* finally asked for the copyright for all reports on my rocket flights, and reporters appeared to take photographs. I was personally distressed that my postal rocket successes received so little recognition.

I want to say a few final words on my experiment V-8, which took place on October 28, 1931. I launched the rocket with a selen cell as an optical control which should have set its course toward a lighted balloon. This rocket was intended as a preliminary test for future flights towards a star or an airplane containing a light beam. Unfortunately, this night flight failed.

I continued building postal rockets until 1935. They were constructed as group and step rockets as well, but they never exceeded the weight of 30 kg because I could not afford to build bigger ones. Though each was built differently, all of them were intended to advance the cause of space flight. Later, I destroyed nearly all of my research notes and photographs of rocket launches and proceedings, for fear they might be used by the military. I abandoned space research entirely after World War II.

In the years that have followed, rocketry has gone through impressive advances, best reflected perhaps by that enormous success of science and technology: manned flight to the moon.

ROCKET FLIGHT TO THE MOON--FROM IDEA TO REALITY: A MEMOIR[+]

Rudolf Nebel (German Federal Republic)[++]

There is no stronger force in nature than an idea whose time has come. About the turn of the century, it was the idea of flying. Thus, the "journey to the Moon" had already begun in 1908 when I bought for 25 Pfennig in Nürnberg, Jules Verne's book The Journey to the Moon that described how three men were to be shot to the Moon in a giant projectile from a cannon near Cape Kennedy. However, I soon figured out that this was impossible, since all three would have been killed immediately when the projectile was fired. I took Verne's book to my physics professor, Dr. Hess, at the high school in Nürnberg, and asked him how and whether a person could actually fly to the Moon. After reading this book, he said that he did not know either. But Verne gave a figure of 11.181 km/sec as the velocity that a body must have in order to leave the gravitational attraction of the Earth, and that pointed to rockets! But what was known at that time about Rockets?

We knew that the Chinese had invented rockets in 800 AD as incendiary arrows, and that German rocket corps had played a role in the War of Liberation (1813-1815). When rifled guns came on the scene, military rockets disappeared, in general consigned to fireworks and signal applications. Professor Hess advised me to get involved with flying so that I could get an idea for myself of how and whether we could actually get to the Moon. But what did we know about flying at that time? I found out that Otto Lilienthal had already made more than 2500 flights in the years 1890-1896 in a glider he had put together himself, thus laying the foundations for present-day powerless and powered flight. Thus, at the start of the century, powered flight began in Berlin-Johannisthal (Figure 1).

I wanted to see this powered flying for myself, close up. Now I did not just go out and buy myself a train ticket, although a fourth-class ticket from Nürnberg to Berlin and back would have only cost 5 Marks. Instead I built myself a bicycle for

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[+] Presented at the Fourth History Symposium of the International Academy of Astronautics, Constance, German Federal Republic, October 1970.

[++] An early experimenter with solid-and liquid-propellant rockets in the 1920s and 1930s.

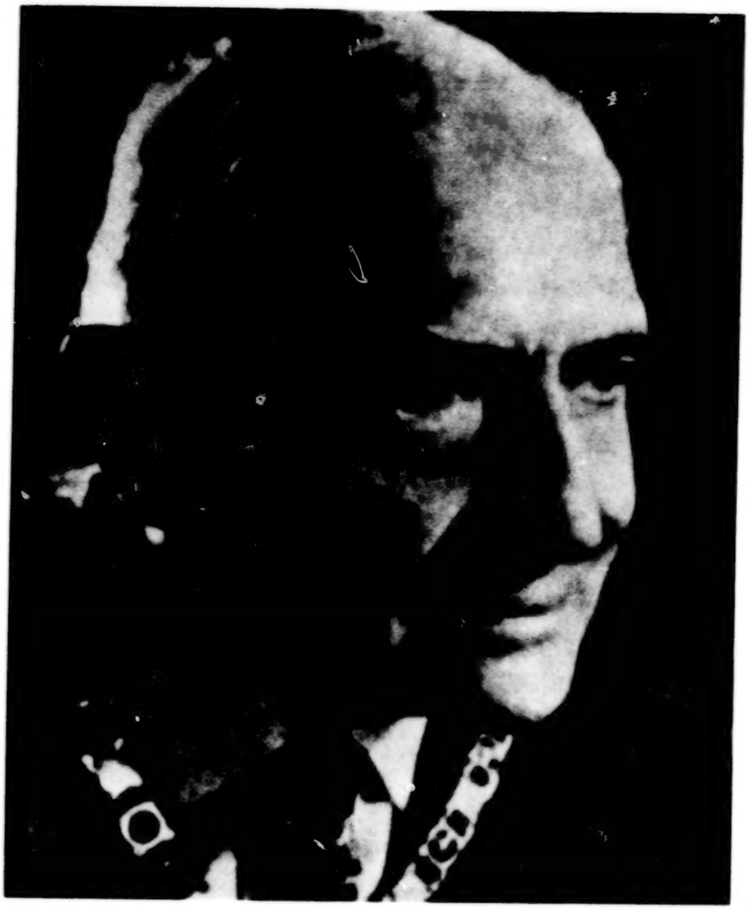

Fig. 1
Rudolf Nebel

5 Marks (Figure 2). In order to use it, I had to pass a bicycle test in front of the 16 Aldermen of Nürnberg, whereupon my bicycle acquired the number 18351; now I could ride it to Berlin. Five hundred kilometers going, five hundred kilometers back on dusty streets, full of horseshoenails, and plagued by dogs which chased after me--what an adventure!

In Johannisthal I then saw the first airplane, which flew around at housetop height when there was no wind at all. This made me so enthusiastic that I immediately resolved to take part in flying. Naturally, I needed something which would require only a minimum of expense, and so I decided to build a large kite which would carry me aloft and then glide back to the ground. However, once I had built the kite, I was forced to admit that it was not at all capable of lifting me off the ground. Instead, I learned that I would have needed [illegible] such kites. However I did not have the money. But I got the money in an original [illegible]. I built myself a card-board camera, installed it in this kite, tripped the shutter with a string--an elastic cord--1000 m above Nürnberg, and

Fig. 2
Home-Made Bicycle No. 18351

obtained the first aerial photographs of the town from the kite. At that time, I took more than 200 such aerial photographs. They sold like hotcakes, so I no longer had to consider large kites, but instead had the money to build a genuine monoplane (Figure 3), such as I had seen in Johannisthal.

When it was finished, I rolled around the training ground in Nurnberg in 1911. Now, procuring money with photographs was naturally very arduous, so I wrote to the War Ministry, stating that it would someday be possible to utilize such aircraft for military objectives, and requested financial assistance in my work. The reply was a disappointment. They responded that aircraft could never have military significance, because piloting an airplane required acrobatic agility.

On August 15, 1912, I won my Bavarian pilot's license No. 178 with a flight from Nurnberg to Frankfurt and back. Within 2 years, moreover, the War Ministry had to admit that war could not be conducted at all without aircraft. But because of this short-sightedness, Germany arrived at the front with 750 old Taubes, while the French already had 3000 new airplanes, securing air superiority for the enemy from the outset (Figure 4). However, the War Ministry soon received even more dramatic proof of the necessity of airplanes. At the beginning of September 1914 our flyers returned with the information that the French were retreating in flight toward Paris, but they were ridiculed because the cavalry had reported the French advancing in full strength toward the Marne.

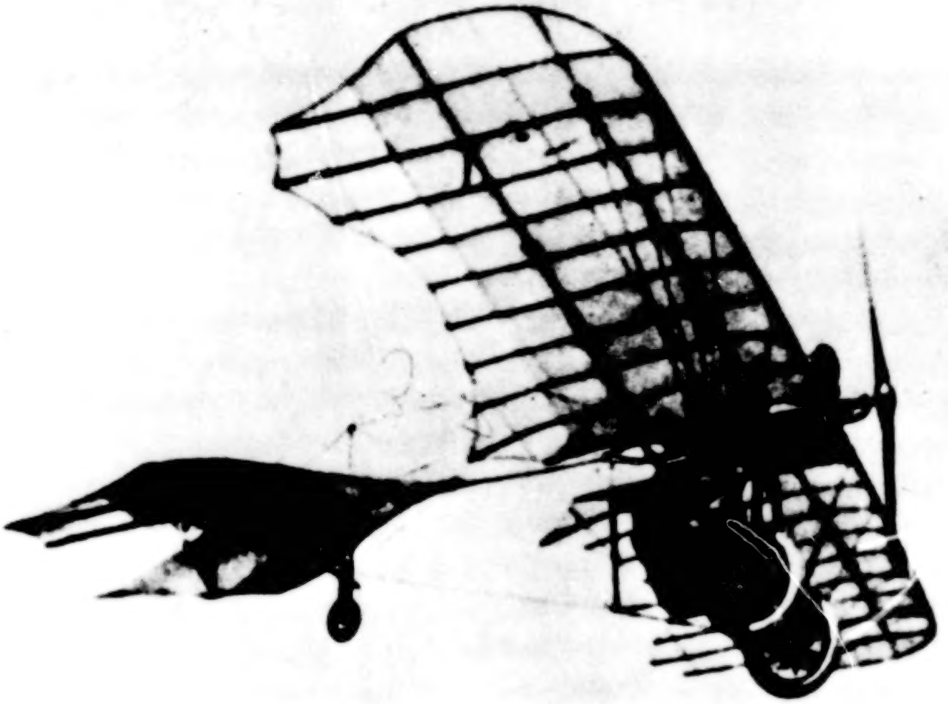

Fig. 3
Home-Made Airplane

A General Staff Officer was summoned from the Central Headquarters in Spa to make a decision. He decided for the report of the cavalry and used his authority to order a retreat to the Marne. But what would have happened if we had conquered Paris at the beginning of September in 1914? In hindsight, we can only say that the entire First World War would have gone differently. Now the War Ministry recognized that airplanes had to be built in great haste and that we had to fly them!

In 1916, I sat in the cockpit of a Focker monoplane (Figure 5) with two machine guns that fired through the propeller and were supposed to shoot down enemy airplanes. I soon found out that things were not this simple. With these primitive machines, one had to fly within 20 m of an enemy aircraft in order to hit it at all. However, the air superiority of the enemy was about 10:1. Before I could get within the 20 m, my own craft had been hit. Shards flew, the machine went out of control and into a steep dive. When I woke up, I was in a field hospital covered with bandages. I now had time to consider whether it would not be better to shoot down enemy airplanes with rockets from a distance of 200 m, instead of with machine guns from a distance of 20 m.

When I was released from the hospital, I went to the nearest engineering-supply depot, drew out the largest signal rockets, and attached warheads to them. Everyone was tensely waiting to see what would happen when, 4000 meters up, we encountered an enemy

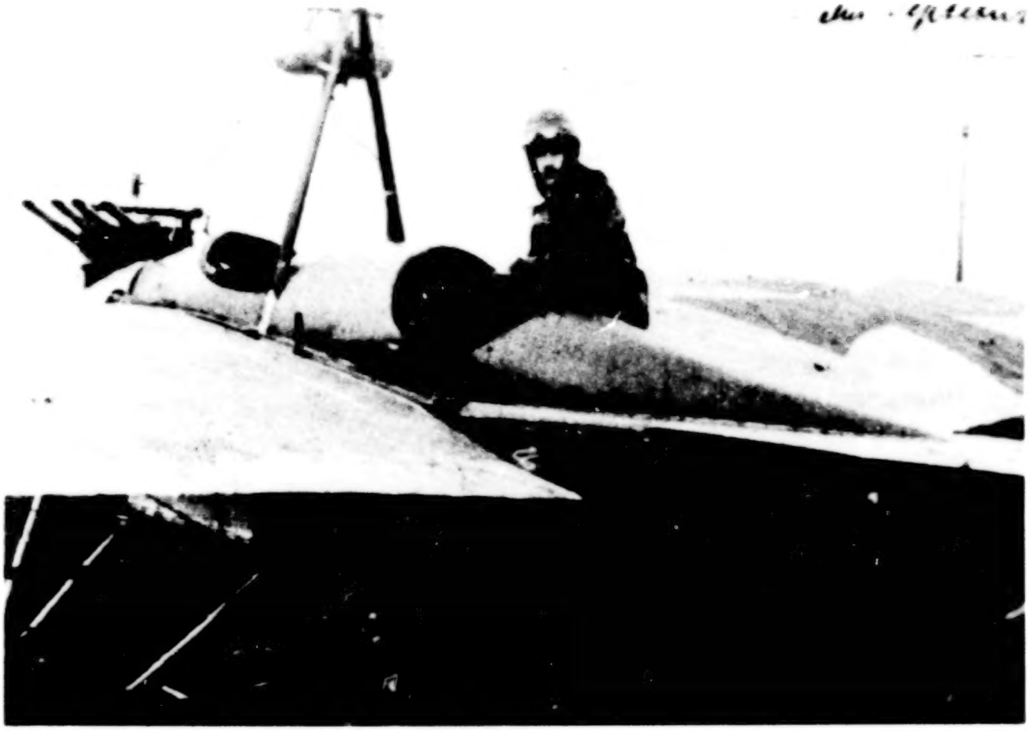

Fig. 4
Rudolf Nebel in a Taube, 1914

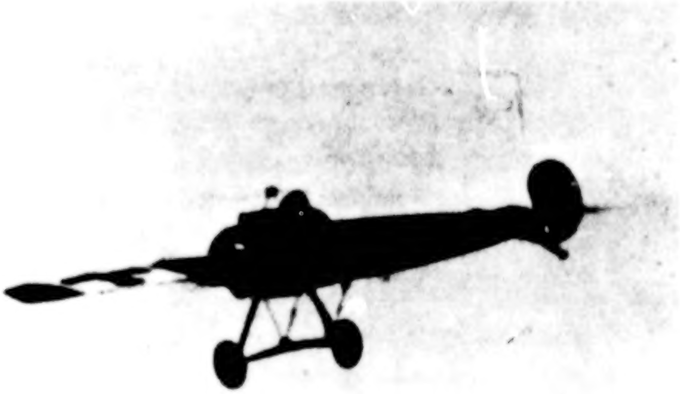

Fig. 5
As Fighter in Fokker Monoplane

squadron of 25 planes. I pressed the button; an immense trail of powder smoke passed through the center of the enemy squadron. One plane immediately dropped its nose and went into a dive, landing on the nearest meadow. With the second hit, I succeeded in shooting away an enemy propeller. Only with the third shot did I get into trouble: I shot myself down. When I pressed on the button, the primitive hand-made rockets exploded before they had left my airplane. The aircraft caught fire--and there weren't any parachutes at that time.

I plummeted toward the earth in my burning machine. But then I had an improbable stroke of luck. Right next to where the burning aircraft struck the ground, two privates were working on line construction and they got me out of the burning aircraft in time. I certainly would not have escaped by myself. They delivered me, somewhat burned, to the hospital. I was immediately commanded to cease using these new weapons. However, we had to have a new name for this weapon. We baptized them "Nebelwerfer" (Editor's note: this pun on the author's name means literally "smoke thrower"), and still tried unsuccessfully to get the Nebelwerfer used in the First World War. We had to continue fighting with machine guns. In 1918 I acquired the best fighter plane of the First World War, a Pflaz D 8 (Figure 6) with a 300-hp rotary engine with which I could climb to 8000 meters in 30 minutes. The air battles at elevations of 8000 meters taught me that I could take these aircraft up only as far as there was still air. If one wished to fly higher into

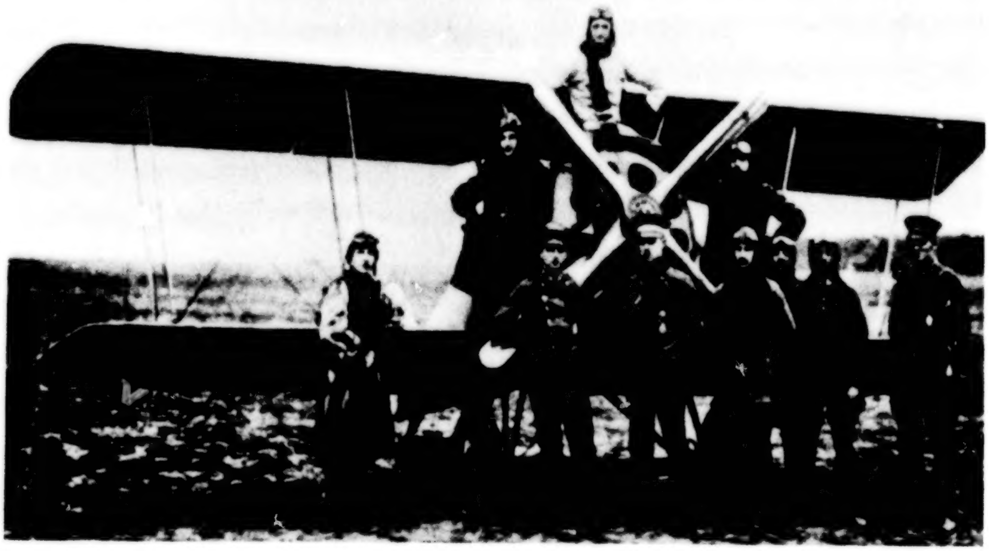

Fig. 6
As Fighter in Pfalz D 8 with 300-hp Rotary Engine

airless space, one required an entirely different type of drive, namely jet or rocket drive which I later patented as the liquid-propellant jet engine under the number DRP 633,667.

After the War, I initially had other worries. I received my engineering degree at the University of Munich in 1919. In order to finance further rocket research, I founded a fireworks factory at Pulsnitz in Saxony. I sold fireworks to rifle clubs and continued my research on rockets. With some comrades who had survived World War I, I believed that we had to do something to prevent a second World War. One contribution could be a rocket which would fly at least 1000 km. We chose the slogan "no more war," and founded an International Research Society that later received the name "PANTERRA." Our objective was to stimulate the interest of the peoples of the Earth in the major problems of science and technology, and divert unnecessary funds for armements to peaceful and productive work. We organized this world peace program of PANTERRA as follows:

1. Rocket flights for the purpose of space travel.
2. Atomic energy for peaceful purposes.
3. Robots to relieve mankind of manual labor.
4. Geothermal power plants for exploiting the heat of the Earth.
5. World power plant Gibralter--lowering the Mediterranean Sea.
6. Making fertile the Sahara Desert.
7. Large-scale wind power plants.
8. Tidal power plants.
9. Solar mirrors to influence weather.

In 1927 the Society for Space Flight was founded, and in 1928 the first rocket cars were driven on the Avus test road. In 1929 UFA filmed a movie "Woman on the Moon" that employed Hermann Oberth as scientific advisor. He had already published the book Rockets to the Planets in 1923. I met Hermann Oberth on the UFA lot, and this meeting turned rocket flight into reality. UFA provided 35,000 Marks for the first liquid-propellant rockets built at IG Farben in Bitterfeld, brought about the first combustion tests with liquid oxygen and gasoline, and resulted in a meeting (Figure 7) on July 23, 1930, with personnel at the Reich Chemical-Engineering Laboratory in Berlin.

Thus, on September 27, 1930, I established the first launch site in the world in Berlin-Reinichendorf. Starting with minimum rockets with one liter of liquid propellants, abbreviated MIRAK 1, and through four liter, 50 liter, and 500 liter rockets, the theoretical foundations of the V-2 rocket were worked out. This work resulted in the patent DRP 633,667, a jet engine for liquid propellants, and the DRP liquid propellant rocket as a secret patent with the number N 32827 I 46g. The first liquid-propellant rockets driven with liquid oxygen and gasoline were launched from an island in the Tegeler Sea. A manned rocket, the "Magdeburg piloted rocket," was also built, financed by the Mayor of Berlin, Professor Reuther. It did not get beyond a test launch in

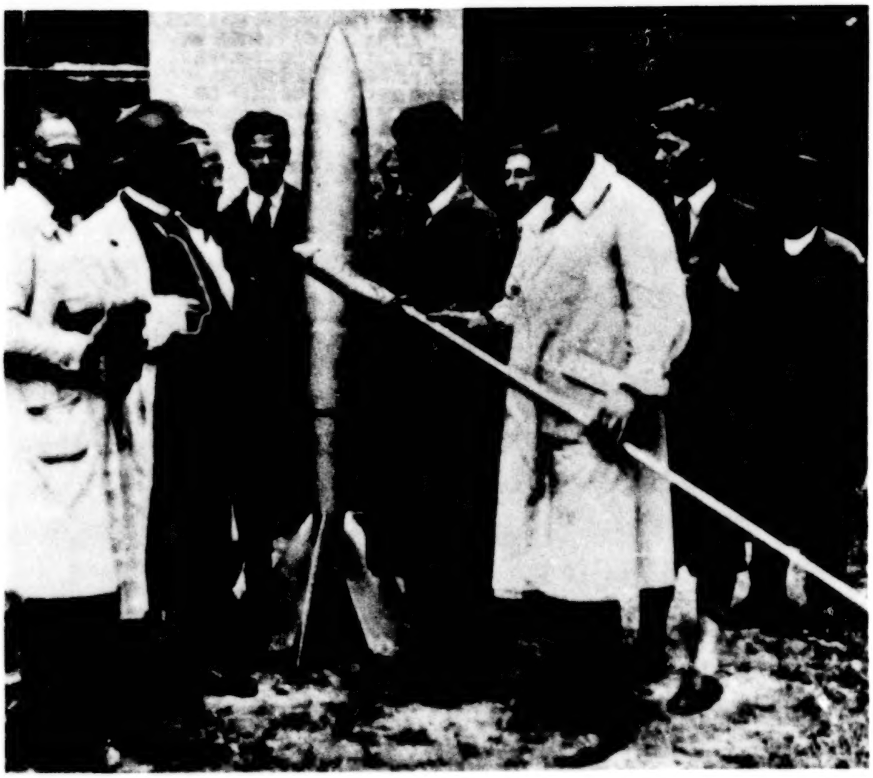

Fig. 7
Experts from Chemisch-Technische Reichsanstalt
[Reich Chemical Engineering Laboratory]
with Nebel, Oberth, von Braun, and Dr. Ritter

Magedeburg, because on January 30, 1933, Adolph Hitler came to power; the rocket became a "secret command matter," and the tests were transferred to the authority of the Army Weapons Office.

The Reich Central Security Office (RSHA) in Berlin took over supervision of the rocket launch site in Berlin. I succeeded in convincing them that it was important to put together a documentary film to record the experiments for posterity. This was possible because the Siemens Company had brought out the first 16mm film device in 1930, and I got the idea of filming the experiments from the beginning. The documentary film "Rocket Flight 1944" was produced, but confiscated by the English on May 2, 1945. Through my personal visits to London, I eventually succeeded in obtaining the release of this documentary film and had it deposited in the Federal Film Archives in Koblanz.

On May 2, 1945, the Americans arrived at the V-weapons plant at Neder-Sachswerfene in Harz, took possession of the last 18 V-2s and V-1s, and all the German patents. Some 200 rocket researchers led by von Braun were also taken to the USA, where they later developed other liquid-propellant rockets including 15 Saturn V rockets. A spacecraft launched on the seventh Saturn V landed a man on the Moon on July 20, 1969.

But despite all the outstanding outerspace achievements made possible through the principle of rocket propulsion, I already foresaw in 1932 great difficulties in placing a large manned spacestation in orbit by rocket propulsion alone. Therefore I welcomed a study made by Mr. W. H. Kurpanek about a method to place a space vehicle into orbit by electromagnetically accelerating it in a evacuated 10-mile-long tube (Figure 8). We submitted the study in 1969 to NASA. At the Marshall Space Flight Center in Huntsville, Alabama we received a research grant of $280,000 to build a electromagnetic accelerator tube for simulated meteor impact studies. The Metro Physics Company in Santa Barbara, California, was chosen to build the accelerator tube but the company failed to reach a business arrangement with NASA, so we postponed this project.

The mounting difficulties with the Space Shuttle Project, the launching of a large spacestation and most of all the accumulation of radioactive waste material prompts us to show the most economical way to solve those problems. A electromagnetic accelerator is the most economical and clean way to launch things into orbit, for only the payload in a hull without any instruments or motors on board is brought into orbit. The needed energy is produced on the ground with a MHG-Generator.

A half century ago I initiated with others the first generation of space flight; man has reached the moon, now I would like to initiate together with Mr. W. H. Kurpanek the second generation of space flight and let man reach the planets.

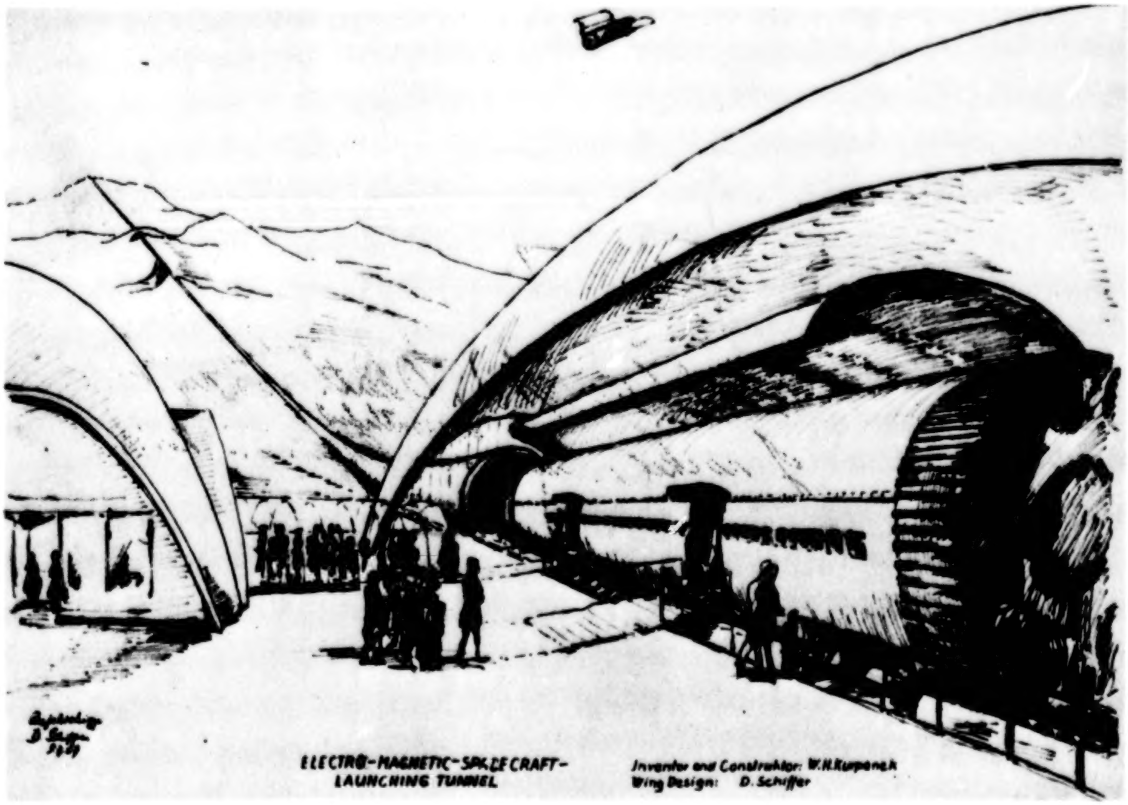
ELECTRO-MAGNETIC-SPACECRAFT-
LAUNCHING TUNNEL
Inventor and Constructor: W.H.Kurpanek
Wing Design: D. Schiffer

Fig. 8

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ORIGINS OF ASTRONAUTICS IN SWITZERLAND[+]

Alfred Waldis (Switzerland)[++]

Switzerland has no claim to a pioneering role in astronautics, nor is our country among the leading nations in this field today. Also, Swiss contributions cannot be measured on the scale of those made by larger countries. Yet, on taking a closer look, one discovers some daring and way-breaking thoughts and experiments originated by Swiss scientists and researchers.

The task of setting a date on the origins of modern astronautics can be rather difficult. As part of the natural sciences, astronautics built upon all of the major mathematical, physical, and chemical principles—all closely related to most other branches of research. Two early Swiss scientists, for example, made basic contributions to astronautics: Daniel Bernoulli, a mathematician from Basle, with his principle of flow (Hydrodynamica, 1783), and Leonhard Euler (1726) with his textbook on differential calculus.

Legend has it that each of the famous Bernoullis lived in the hope that he would create an abstract mathematical principle that had absolutely no application. Five of them died disappointed; someone always found good use for every concept they produced. The sixth one, it is said, died happy because he thought he had finally produced a principle that was thoroughly useless. That was the theory of the solution of simultaneous equations (now the basis of so much of our advanced mathematics in daily use all over the world). Whether or not this legend is apocryphal, it does illustrate a point to be kept in mind: any mathematical discovery, in the long run, will likely contribute to the solution of some problems that are the concern of man.

Before reviewing the modest contribution to astronautics made by our small, neutral country, it would be useful to mention the reasons why space technology to this day has not attained its due recognition in Switzerland.

[+] Presented at the Sixth History Symposium of the International Academy of Astronautics, Vienna, Austria, October 1972.

[++] Director, Swiss Museum of Transport, Lucerne, Switzerland.

- In larger nations, the development of launch vehicles for artificial satellites and space probes was made possible largely by armament or highly-specialized aeronautical industries. In the period during and immediately after World War II, Switzerland had neither an armament nor an aeronautical industry capable of such a task.
- Aerospace developments in today's leading space nations depended on substantial government grants or subsidies. Switzerland traditionally relies entirely on private industry for applied research. It was not possible, within existing laws, to create a national space program providing the necessary funds for even a modes research program.

In his opening address at the Fourth Congress of the International Astronautical Federation in Zurich in 1953, the well-known Swiss professor of aerodynamics, Josef Ackeret, very accurately summed up the essential considerations for the cooperation of individual researchers and small nations in space projects: "For young enthusiasts the situation is frustrating. While in aeronautics one or a handful of amateurs can by themselves build at least some primitive kind of flying machine with which to experiment, this is all but impossible in astronautics where activities seem to be restricted to 'theoretical hobby craft.' Real development work," Professor Ackeret continued, "can only be realized on a truly large scale. And this scale is bound to be so big that not even the largest companies, let alone private individuals, could embark on the venture. Here, an entirely new form of teamwork is required. Each nation and each individual must fit in a scope within which he can make a useful contribution." These thoughts suggest the limits to the range of space activities in my country to the 1950s.

FIRST STRATOSPHERE FLIGHT

One of the pioneers who tried to widen this dimension was the experimental physicist, Professor Auguste Piccard. Let us examine a few interesting details of Piccard's historic balloon high-altitude flight of May 27, 1931. This event captured world attention in a high-altitude flight for scientific research. Together with Engineer Paul Kipfer, Piccard ascended to nearly 16,000 meters to conduct radiation measurements. The spheric cabin was made of 3.5 mm-thick aluminum plate, and consisted of three forged parts assembled by autogenous welding. Eight vertical supports in the interior formed the frame and supported the crew. Professor Piccard's ascension had its dramatic moments.

At take-off, a sudden gust prematurely swept the balloon out of the hands of the launching crew before the take-off weight was under control and the instruments properly stowed away. Adding to the troubles, the revolving device for the cabin did not work; as a result, the dark outer side was exposed to direct sun light, while the reflecting aluminium side remained in the shade. Temperatures in the upper part of the cabin reached 41° C, while the researcher's feet froze in -10° C. Professor Piccard made the following entry in his log-book: "The situation is serious. There are various dangers. We shall not be able to time our landing as we choose (the valve line had not unwound properly). If there is a leak in the cabin, we shall die of suffocation. It depends on the wind if we drop into the Adriatic to-night or have land below us." Leaks in the cabin actually occurred several times and had to be repaired in a hurry. The scientific measurements were considerably complicated by this fact. At 9 p.m. the cabin carrying the two scientists hit the ice of the Gurge glacier.

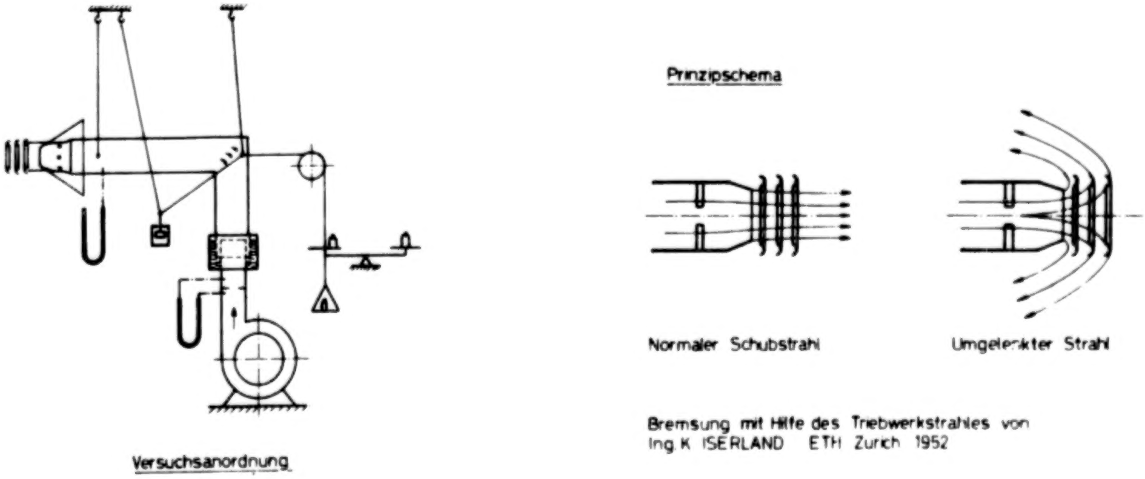
AERODYNAMIC CONTRIBUTIONS TO ASTRONAUTICS

In the field of aerodynamics at high speeds, as applied in rocket technology, Professor J. Ackeret has been Switzerland's leading scientist. His basic paper of 1928, "Air Resistance at Very High Speeds," not only gave an excellent survey of the state of this particular field at that time, but suggested a program of future research. Ackeret was convinced that the future of manned flight lay in aerodynamics of highest speeds. It was he who coined the term "Mach Number" in honor of the celebrated physicist, Ernst Mach. Ackeret wrote: "In aerodynamics of higher speeds, the proportion v/a comes up time and again (v = speed of the examined body, or the airflow, respectively; a = speed of sound). A short term therefore seems indicated. Since the well-known physicist Ernst Mach recognized the basic significance of this proportion with particular perception, v/a may justly be termed 'Mach Number.'" "Mach" has been used ever since in scientific language as well as in popular texts.

Ackeret decisively influenced the development of aerodynamics of high speeds with his publications on gas dynamics, aerodynamic forces on wings, boundary layers in compressible flow, and rocket theory. He began to read on rocket technology in 1941-1942. His paper, "Comments on the Rocket Theory" (see references) finally marked Swiss engagement with questions of space. To Ackeret's way of thinking, such a step was a natural conclusion, and he later gave the opening address at the Fourth Congress of the International Astronautical Federation in 1953. In 1956, he published an article on the "Limits to the Attainability of Remote Celestial Bodies." In it he observed: "The majority of the following remarks were written several years ago, but remained in a drawer, since the conditions for attaining high speeds (comparable to the speed of light) seemed to me all too vague." From mathematical-physics he drew these conclusions: "It becomes clear, therefore, that despite the most daring assumptions, we can no longer think of space travel beyond the nearest fixed stars. A journey of this kind would only make sense if it could be hoped to discover and observe unknown planetary systems. Perhaps forms of life--very different from ours--could be found there. Thus, biology could gain from such a discovery. But one must ask whether more is not to be gained by terrestrial research which actually is only on the threshold of exploring life. Astrophysically, the yield would presumably be modest, since 99% of the substance is in the form of gas. An astronomical observatory on the Moon will furnish all the results that can be expected in this regard. So we may have to resign ourselves to the old saying:

"To tread upon the infinite,
keep going in all ways of the finite."

The construction of the first large closed-circuit transonic wind-tunnel at Professor Ackeret's Department of Aerodynamics of the Federal Institute of Technology in Zurich, in 1933-1934, caused a sensation in scientific circles and opened the way for much more work in the field of rocket aerodynamics. Two papers appear representative of the great number of publications: "To the Theory of Aerodynamic Forces on Slim Profiles and Slim Bodies of Revolution," by H. R. Voellmy, and "Of Toroidal Wings," by Z. Plaskowski. Here, K. Iserland's experiments in 1952 with thrust reverse must also be mentioned. He placed a number of semi-toroidal turn-back rings at the end of a rocket nozzle. Before being ejected from the nozzle, the gas jet is set spinning by a circle of revolving blades. As a result of the centrifugal load, the jet spreads out, hits the rings and is turned back by them. If the blades are set in the direction of flow, keeping the jet free of spin, the gas is ejected from the nozzle in a straight line and flows through the centre of the rings, without changing direction (Figure 1).



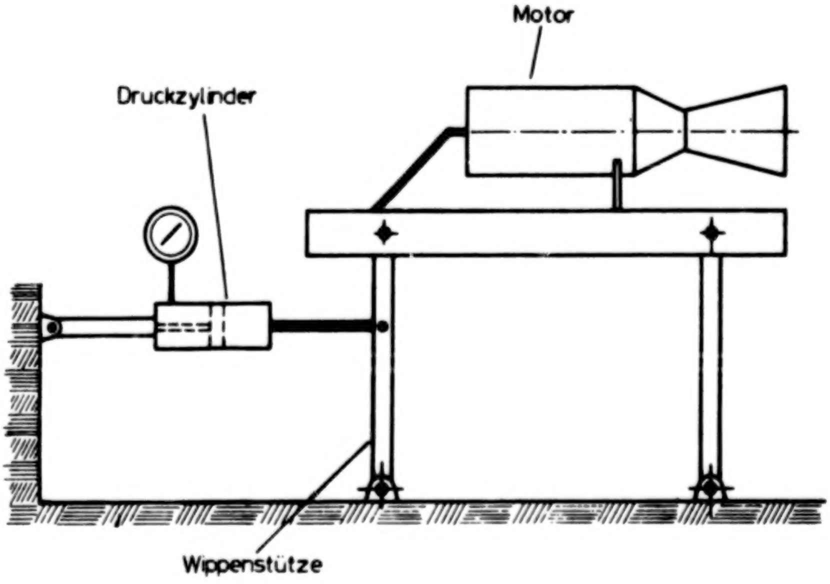
Versuchsanordnung

Bremsung mit Hilfe des Triebwerkstrahles von Ing.K ISERLAND ETH Zurich 1952

Fig. 1

ROCKET EXPERIMENTS

Another farsighted Swiss pioneer in the field of rocket propulsion was the engineer, Josef Stemmer. After many minor experiments beginning in 1925, he succeeded in 1934 in realizing what he called "a reasonably successful form" of test stand for a reaction thrust engine. In the period between 1934 and 1945, Stemmer build various rocket engines and test stands. The first experimental rocket engine had two combustion chambers and two ejection nozzles. Propellants were injected separately from the nozzle side in the opposite direction of flow. Given very limited funds, Josef Stemmer's experiments had to be carried out on a very modest scale. With the exception of the combustion chambers for the first test stand which came from an old two cylinder Mercedes gasoline engine, all the parts he used, such as combustion heads, injector and ejection nozzles, were home made (Figure 2). His second rocket engine propelled a model airplane. A third was fitted with a mixing injection nozzle and a special cooling jacket. It served to study problems of cooling and fuel. The fourth rocket engine had a changeable combustion



Raketen-Prüfstandschema von J. STEMMER 1945

Fig. 2

chamber and a special coolant supply system. It was used to test ejection nozzles and to determine the duration of gas presence.

Stemmer continued his experiments with flying models and test stands until 1945. In a series of publications entitled "The Development of Rocket Propulsion in a Generally Understandable Presentation" (1945), he proposed a development program intended to be applied on a larger national scale. In 1951, Stemmer was appointed Secretary of the International Astronautical Federation. At the end of the same year, he founded the Swiss Astronautics Association.

SPACE SCIENTISTS

Beginning in 1932, another Swiss scientist, Professor Dr. Jakob Eugster, concentrated his studies of the effects of cosmic radiation. His investigations were first conducted jointly with Victor Hess, who discovered cosmic rays on his balloon flights in 1912. The studies were pursued at Innsbruck (Hafelekar, at 2340 meters altitude, and in the former salt mines of Hall), and, after 1937, at Fordham University in New York. These studies concentrated on the effects of cosmic radiation on unicellular organisms and on healthy and diseased human tissue, and, more recently, in physiological form, with astronauts. Since 1950, a special observation station for biological controls has been operated in the interior of the Simplon tunnel. Professor Eugster used the so-called "sandwich method," in which the objects to be examined--seeds of plants or ova of small animals for instance--were placed in the cavities of a plexiglass screen-plate. This preparation was fixed between two photo-plates with nuclear emulsion. The package was then enclosed in a light protection wrap and transported to high mountains, or attached to stratospheric balloons, or carried to high altitudes by rockets. After exposure, the photo-plates were developed; the traces left in the emulsion helped to evaluate the exact location of the hits in the biological matter. The untiring effort of this scientist is reflected in over 100 original publications.

Alfred Stettbacker involved himself with the chemistry of rocket propellants and the production of explosives, among them the most violent of all, "Pentrinit." In his book on explosives published in 1933, he clearly predicted the use of rockets as long-range weapons "in the next war." Another work, published in 1948, included a chapter entitled, "The Rocket or the Long Range Shot," describing the various rocket propellants and adding comments on rocket ballistics.

After the Second World War, rocket development was taken up by Swiss private industry. At that time, solid propellant rockets of up to 350 m/s speed were considered "high speed rockets." In these developments, one tried to achieve speeds of over 600 m/s, with the result that conventional projectile heads were no longer suitable aerodynamically. Consequently, new forms of solid propellant rockets were developed that could be launched with particularly high precision and with a minimum of air resistance. Initially, fully fixed tail units were used, replaced later by adjustable (flap-back) tail units.

Development of a Swiss remote control rocket started in 1946. Aerodynamic tests with this rocket led to the addition of tail unit surfaces for guidance control after completed combustion, and of longitudinally shiftable wings to control changes of the center of pressure. In the course of wind tunnel tests a special measuring device was developed. It worked on the principle of an analogue computer, and helped to reduce wind tunnel tests to a minimum.

Table I, though incomplete, indicates the activities of Swiss scientists in various astronautical fields. This work indicates some of the state of research and the infrastructure for industrial production of parts, components, and complete structures for use in space. This basic research has enabled Switzerland to produce a high-altitude research rocket, developed by Contraves that reached an altitude of 300 km. Satellite structures have been successfully built, and quite a number of components for space vehicles and rockets prove Switzerland's ability to meet sophisticate requirements in specialized sectors. Most Swiss universities are engaged in some type of space research, if only on a small scale, and thus carry on the effort from modest beginnings.

A great many Swiss scientists who were unable to realize their research projects at home have engaged their knowledge and work in American space programs. Representative of these are Fritz Zwicky, Professor of Astrophysics at the California Institute of Technology, and Dr. H. U. Schürch. Professor Zwicky is known as the "Father of Ultra High Energy Propulsion." He succeeded in 1957 with Project Meteor, in placing the first artificial meteorites in deep space.[+] Schürch successfully built a variety of satellite structures.

On the other hand, prominent foreign scientists pursued studies at institutions of higher learning in Switzerland before achieving success in the United States. Among these are Wernher von Braun, who studied with Professor Ackeret at the Federal Institute of Technology (F.I.T.) in Zurich in 1931; Professor R. Bisplinghoff and Dr. John C. Houbolt, who both defended their thesis on technological subjects at our F.I.T. in Zurich. Dr. Bisplinghoff, the Dean at the Massachusetts Institute of Technology, also served in NASA. Dr. Houbolt is known in modern space technology as the "father of the rendezvous technique" used in the Apollo program.

How much Switzerland may contribute in solving aerospace problems in the future remains a question of government support. Today, it does not have a national space

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[+] See Fritz Zwicky, "A Stone's Throw into the Universe: A Memoir," in this volume - Ed.

program. Our parliament is debating grants for ESRO high altitude research and satellite projects. Despite frequent disappointments, the pioneering spirit--shown by Jakob Bommeli of Zurich who lectured on "A Flight Through the Universe" in 1897--has not left Swiss engineers and scientists. But as long as funds are unavailable for the realization of even modest projects, their activities remain limited.

In conclusion, the author must add here sincere thanks to these who contributed to this report, particularly Engineer Eugen Erni of the Institute of Aeronautical Structure and Lightweight Design of the Federal Institute of Technology in Zurich.

TABLE I
Zusammenstellung schweizerischer Wissenschaftler und Forscher die sich bis in die 50-ziger Jahre mit Raumfahrtforschung beschäftigt haben.

Aerodynamik, Antriebe und Energiequellen	Raketenversuche und Raketenentwicklung	Physik der Atmosphäre Erforschung der kosmischen Strahlung	Raketentreibstoffe
Ackeret	Stemmer	Piccard	Stettbacher
Plaskowski	Industrie:	Kipfer	Brunner
Voellmy	Gerber	Eugster	
Gerber	Voellmy	Waeffler	
Iserland	Iserland	Töndury	
Hösli		Jenny	
Grossmann			
Desmeules			
ETH-Zürich		Universität Zürich	
Institut für Aerodynamik		Physikalisches Institut	
Institut für Physik		Physikalische Anstalt Basel	

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THE DEVELOPMENT OF ROCKET TECHNOLOGY AND SPACE RESEARCH IN POLAND[+]

Mieczyslaw Subotowicz (Poland)[++]

FROM THE EARLIEST BEGINNINGS TO THE END OF THE 19TH CENTURY

The first rockets were designed in China after black powder was invented, probably not earlier than the 10th century and not later than in the second half of the 12th century.[1] The first known military application was during a military engagement around Peking in 1232. In Europe, rockets were used for the first time by the Tartars, probably during the battle of Legnica (Dolny Slask - Silesia) in 1241, as reported in a book by the Polish 15th century history writer, Jan Długosz.[2] The Tartars used the poison gases coming out of dragon heads mounted on long sticks. A little church was built on the battleground near Legnica. On the walls inside of the church a fresco shows the battle[3] (Figure 1). T. Przypkowski[3] also found that a monk Seweryn (about 1380), living in the same monastery, had written on the application of powder to propell the "tubes," which probably were rockets. If this is true, it would be the first written statement on rocket application in Poland.

The first Polish description of rocket production appears in the book of M. Bielski, published in 1569,[4] (Figure 2). According to his own experience and experiments, the author provided exact descriptions of how to produce military powder rockets (Figure 3). The next Polish author to deal with rocket problems was Walenty Sebisch (1577 - 1657), from Raduszkowice by Oława (Silesia). He was the military architect of Wrocław. His manuscripts, discovered by T. Przypkowski,[3] date from about the year 1600 and contain sketches of rockets with delta-type stabilizers (Figure 4), conic nozzles in rockets, a rocket battery (Figure 5), and a device similar to the two-stage rocket (Figure 6).

The most outstanding early work is the manuscript of A. Dell´ Aqua (1584 -- after 1654) Praxis reczna działa (Hand Production of Guns). Dell´ Aqua came to Poland from

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[+] Presented at the Sixth History Symposium of the International Academy of Astronautics, Vienna, Austria, October 1972.

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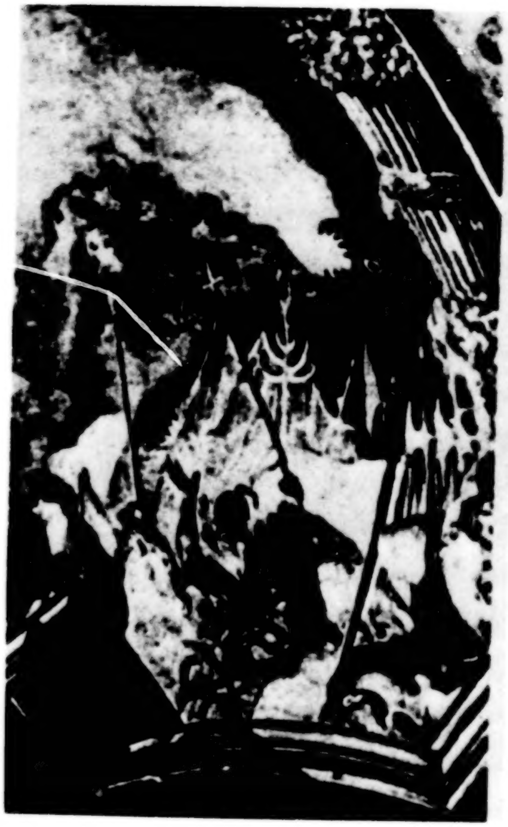


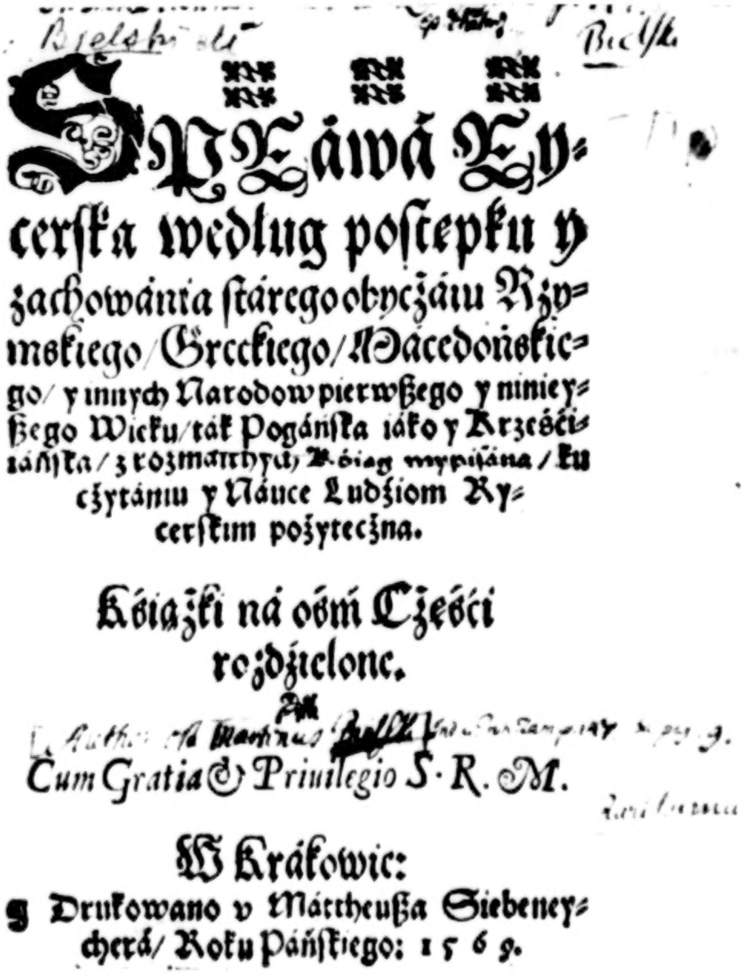
Malowidła ścienne w klasztorze wystawionym na polu bitwy pod Legnicą obrazują używane przez Tatarów głowy smocze miotające ognie i dymy na głowy walczących wojsk. Być może, że dla przedłużenia zasięgu tych dymów i gazów używano również pocisków prochowych lub rakiet. W tym klasztorze przebywający mnich Seweryn w 1380 r. czynił próby „ruchomej rury przy pomocy prochu" będącej prawdopodobnie rakietą na paliwo stałe.

(Foto T. Przypkowski)

Fig. 1

Venice in 1613 and remained in the service of the Polish King and the Polish princes up to the end of his life.[5] His manuscript was written in Polish over a period of five years, between 1630-35. At least five copies survive. In Part III of the manuscript (p. 389, K 187, Cz. 383) is found Figure 171. The rockets G-H are combined in a two-stage rocket (Figure 7). The second stage H, a rocket battery, contains five little rockets.

Kazimierz Siemienowicz occupies a distinguished place in the history of rocket technology. He was made a Polish General of Artillery by the King Władysław IV, and served during the first half of the 17th century. He wrote the well known and outstanding book Artis Magnae Artileriae . . . , published in Amsterdam in 1650,[6] and translated later into many European languages. The chapter concerning rockets contains the sketches of a multistep rocket, a rocket battery, the conic rocket nozzles, and delta-type rocket stabilizers[7] (Figure 8); the multistage combined rocket is shown in Figure 9. It is not certain if these rocket conceptions are those of Siemienowicz. It is possible that all these conceptions are earlier than those of Siemienowicz, Dell´ Aqua, Schmidlap, Biringuccio or Haas.[7] Similar rocket conceptions were advanced, independently it seems,



P. 69: Bielski

Bielski

Spráwá Ry-
cerſka według poſtępku y
zachowánia ſtárego obyczáiu Rzy-
mſkiego/ Greckiego/ Macedońſkie-
go/ y innych Narodow pierwßego y niniey-
ßego Wieku/ ták Pogáńſka iáko y Krześci-
áńſka/ z rozmáithych Kśiąg wypiſána/ ku
czytániu y Náuce Ludźiom Ry-
cerſkim pożyteczna.

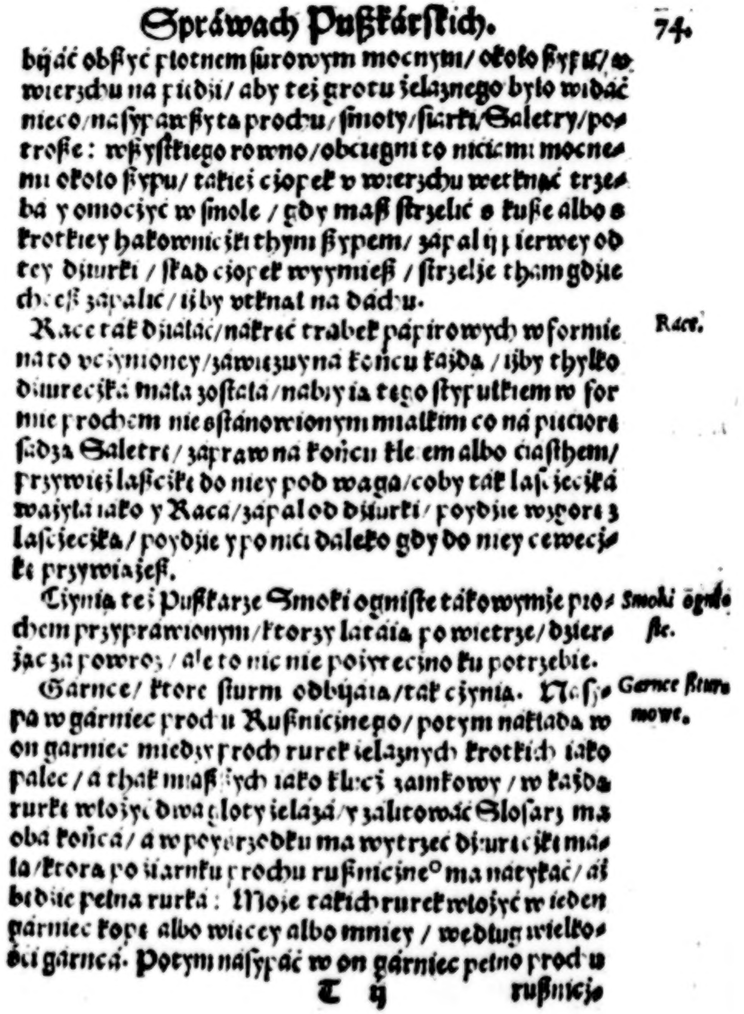
Kśiążki ná ośm Części
rozdźielone.

[illegible] Martinus Bielski [illegible] pag. 9.

Cum Gratia & Priuilegio S. R. M.

W Krákowie:
Drukowano v Mátthеußa Siebeney-
cherá/ Roku Páńſkiego: 1569.

Fig. 2



Spráwach Pußkárſkich. 74.

bijąć obßyć płotnem ſurowym mocnym/ około ßypu/ w wierzchu na pidźi/ aby też grotu żelaznego było widáć nieco/ naſypawßy ta prochu/ ſmoły/ ſiarki/ Saletry/ potroſze: wßyſtkiego rowno/ obciągni to nićmi mocnemi około ßypu/ takież czopek v wierzchu wetknąć trzeba y omoczyć w ſmole/ gdy maß ſtrzelić o kuſze albo o krotkiey hakownicżki thym ßypem/ zápal iy pierwey od tey dźiurki/ ſkąd czopek wyymieß/ ſtrzelże tham gdźie chceß zápalić/ iżby vtknął na dachu.

Race ták dźiałáć/ nákręć trąbek pápirowych w formie na to vczynioney/ záwiązuy ná końcu kożdą/ iżby thylko dźiureczká máła zoſtała/ nabiy ią tego ſtypulkiem w formie prochem nie oſtánowionym miałkim co ná pięciore ſądza Saletry/ zápraw ná końcu kleiem albo ciaſthem/ przywięż laßeczkę do niey pod wagą/ coby ták laſeczká ważyła iáko y Raca/ zápal od dźiurki/ poydźie wzgorę z laſczeczką/ poydźie y po nici dáleko gdy do niey cewecżkę przywiążeß.

Race.

Czynią też Pußkarze Smoki ogniſte tákowymże prochem przyprawionym/ ktorzy latáią po wietrze/ dźierżąc za powroz/ ále to nic nie pożyteczno ku potrzebie.

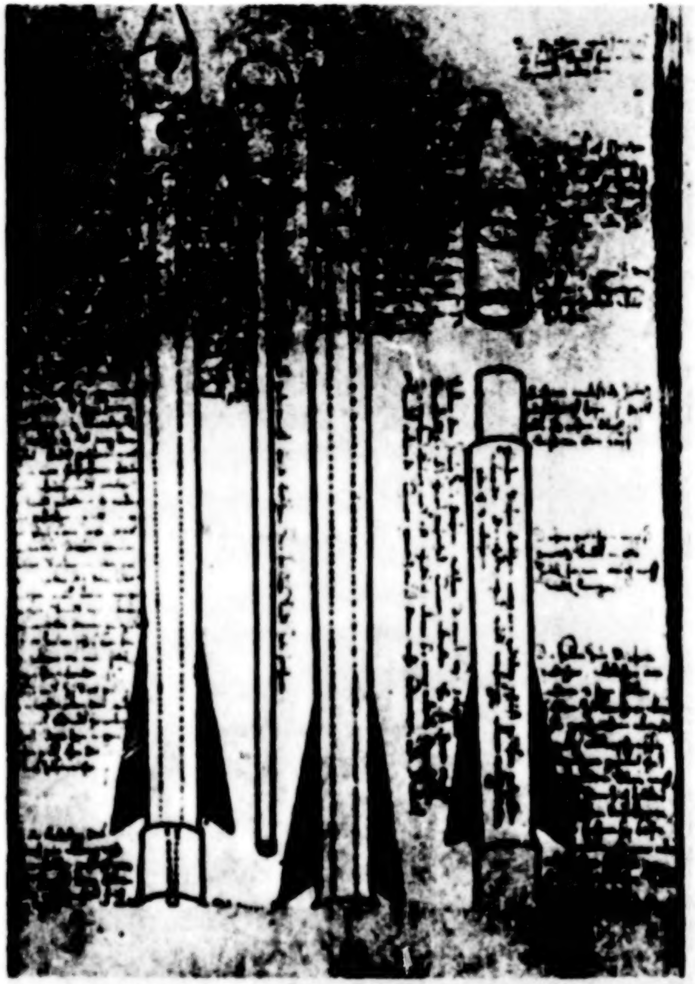
Smoki ogniſte.

Garnce/ ktore ſturm odbijáią/ ták czynią. Naſypą w gárniec prochu Rußnicznego/ potym nakłada w on gárniec między proch rurek żelaznych krotkich iáko palec/ a thak maß ich iáko klucz zamkowy/ w każdą rurkę włożyć dwa gloty żelázá/ y zálitowáć Sloſarz ma oba końca/ á w pośrzodku ma wytrzeć dźiurecżkę máłą/ ktorą po ziarnku prochu rußnicznego ma natykáć/ áż będźie pełna rurká: Może tákich rurek włożyć w ieden gárniec kopę albo więcey albo mniey/ według wielkośći gárncá. Potym naſypáć w on gárniec pełno prochu

Garnce ſturmowe.

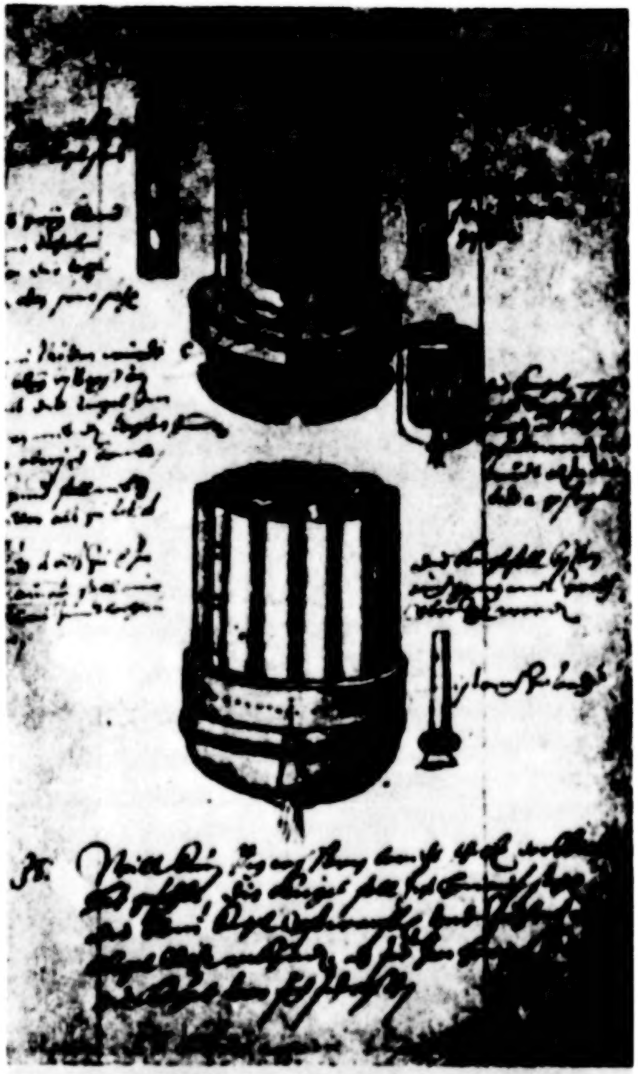
T ij rußnicz-

Fig. 3



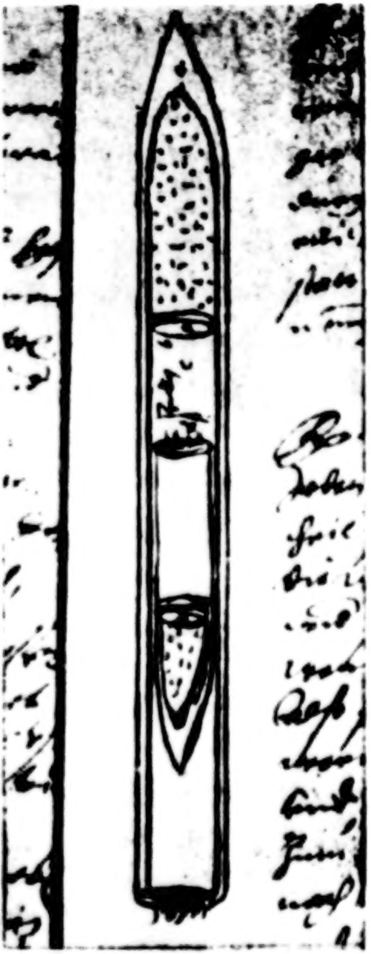
Rys. 9. Własne notaty i rysunki rakiet Sebischa z początku XVII wieku z przyborem do ich wykonywania.

Fig. 4



Rys. 11. Przykład rakiety bateryjnej z notat Sebischa.

Fig. 5



Rys. 12. Przekrój rakiety z korespondencji Sebischa z ukrytą we wnętrzu drugą rakietą, drugiego stopnia, wylatującą jednak w kierunku przeciwnym. Początek XVII wieku.

Fig. 6

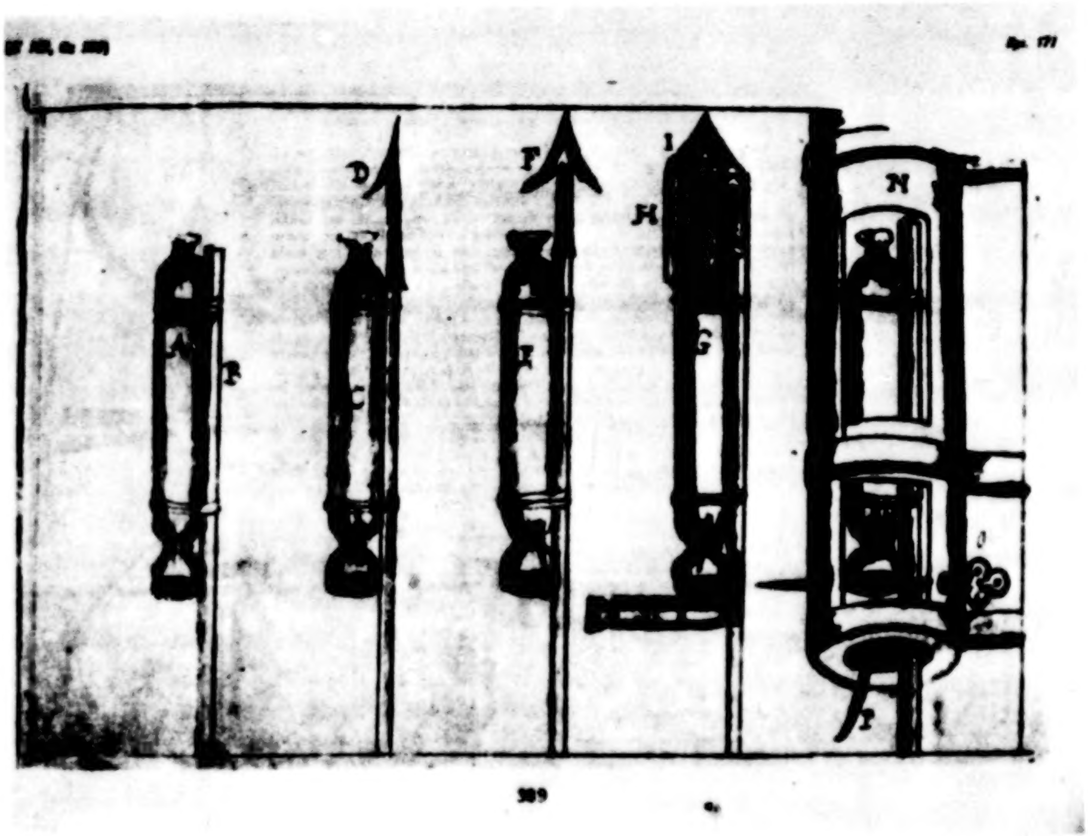


Fig. 7

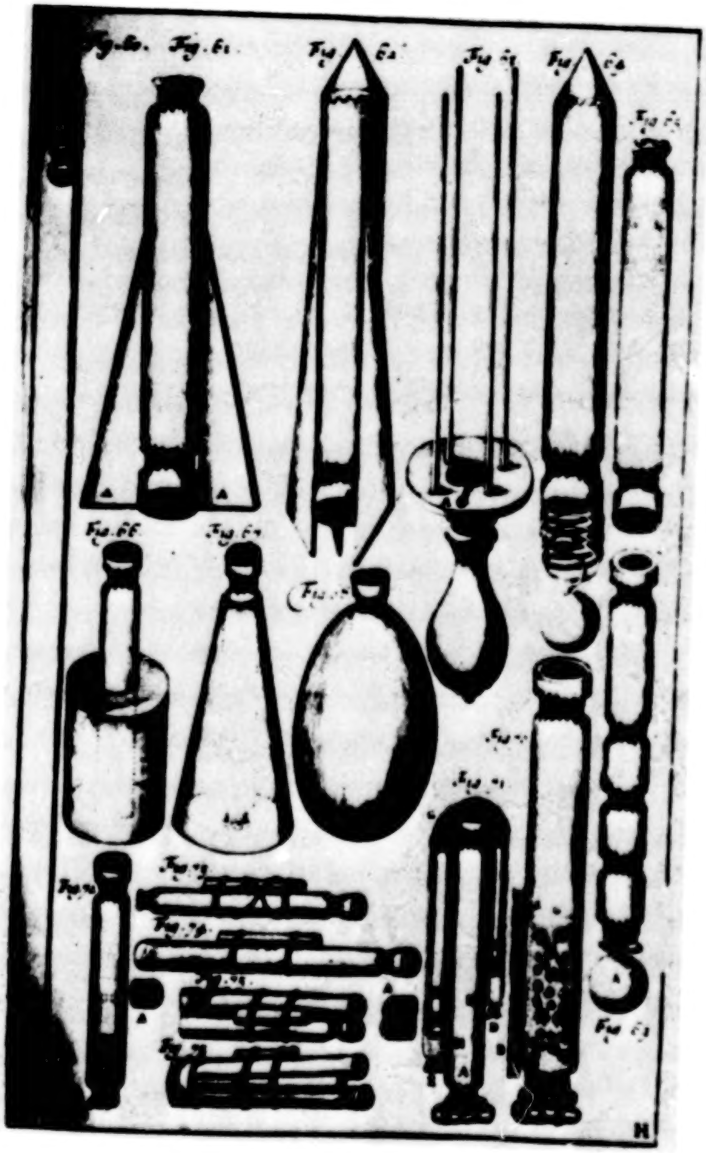


Fig. 8

by different authors before and after Siemienowicz. In any event, his book was the top achievement in the history of the rocket sciences up to the 19th century, namely, to the time of publication of Britain's William Congreve. As far as the priority of the rocket construction ideas is concerned, it is probable that a link between the authors mentioned above and other unknown authors may have existed, e.g., an earlier author.

Besides the ingenious Polish rocket pioneers of the 17th century, we should mention also an excellent forerunner of airplane designers: Titus Livius Borattini (or Boratyni, 1617-1682),[8] who was born in Italy. He came to Poland in 1641 and appeared on the King's court from 1650. Apart from his state activity (as tenant of the Kings

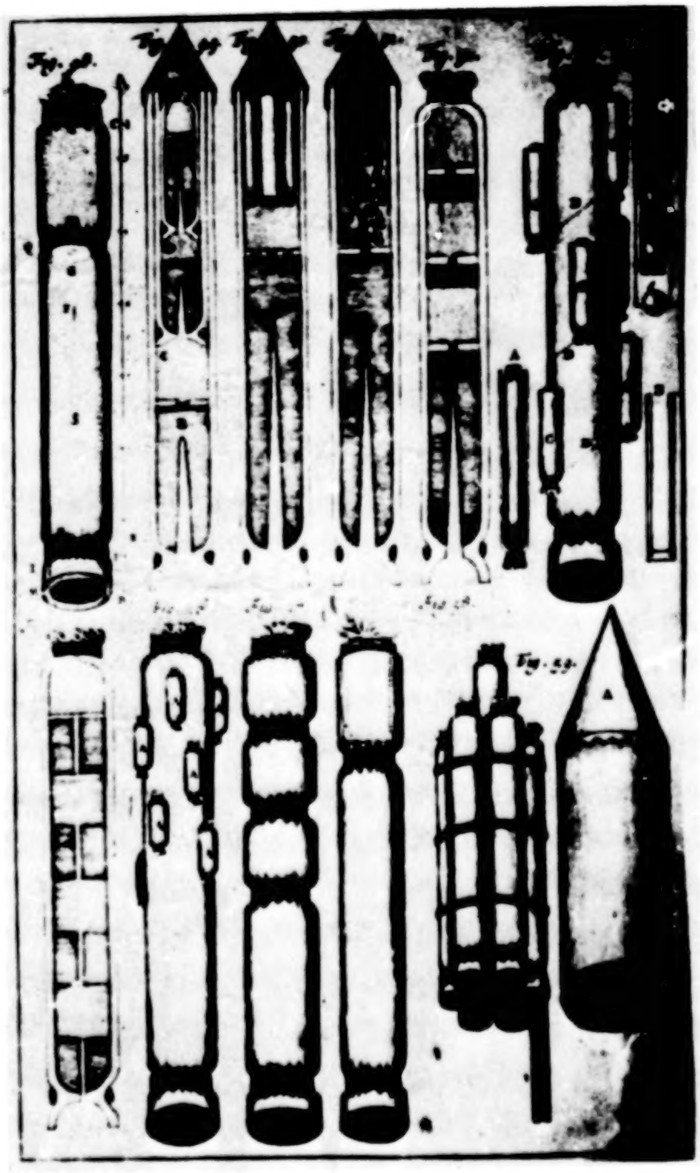
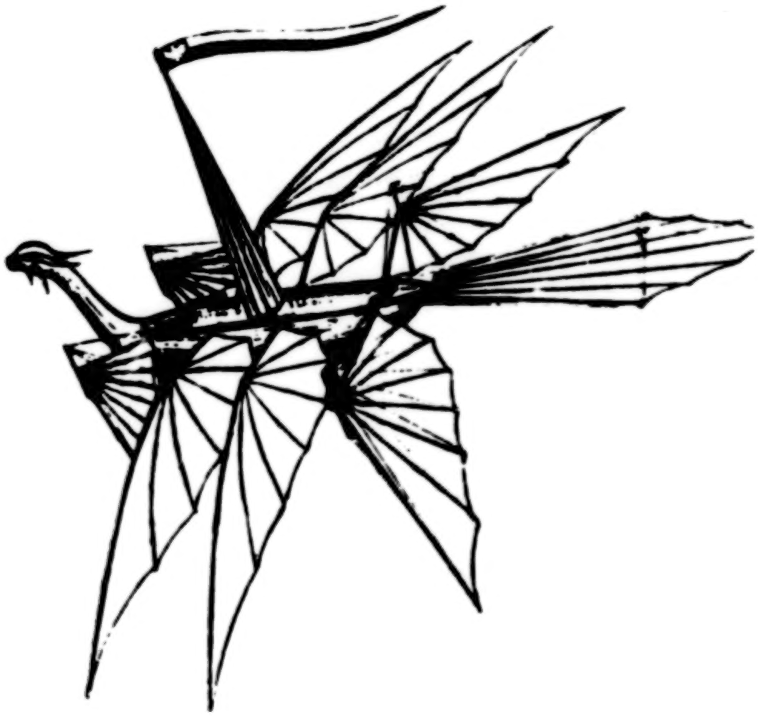


Fig. 9

mint), he also conducted research work in science and technology; he invented a new balance, proposed a new and general system of units in time, length and weight, and constructed models of heavier-than air flying ships (Figure 10). His invention separated two different functions in the flying apparatus: lift force (now supplied by the wing) and thrust force (now supplied by the propeller or the jet engine). The frame of the wings was built of bones and covered with textiles. Human muscle provided the power.

Rockets used in fireworks and for military purposes were described by W. Bystrzanowski, 1749.[9] Later, Captain Józef Bem, in 1820, provided many technological



„SMOK LATAJĄCY" Boratyniego
wg rys. J. B. Cynka.

Fig. 10

descriptions of how to prepare raw materials, machines and apparatus to produce the military rockets.[10] He also summarized the results of field experiments carried out with rocket weapons. Bem gave an account of the bombardment of Gdańsk in 1813 by the English fleet using fire rockets.

LAST YEARS OF THE 19TH CENTURY THROUGH WORLD WAR II

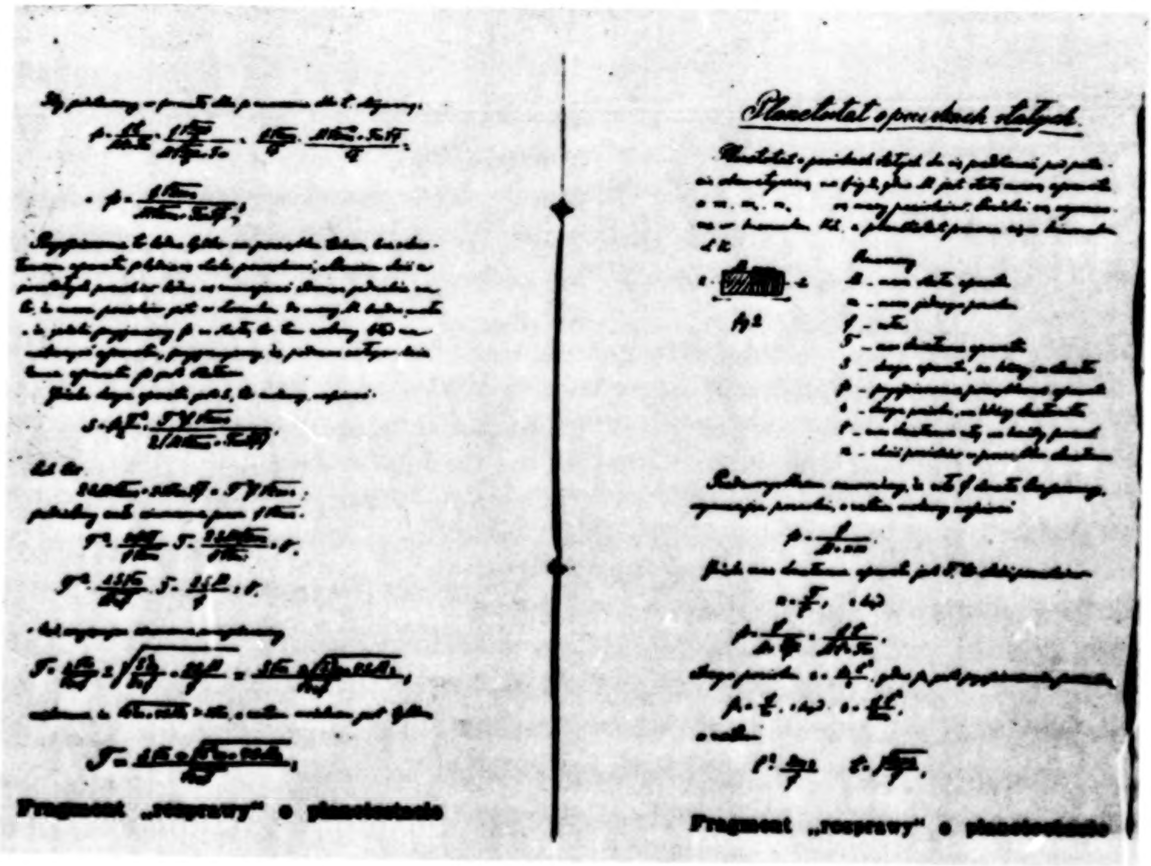
From 1895 to 1903 a young man M. Wolfke (1883-1947), later a professor of physics in the Warsaw Technical University (Figure 11), dealt (in Warszawa) with the problem of communications with other planets. In his first work, Planetostat, he proposed two possible ways of achieving cosmic flight. One used a reaction space ship (planetostat). The forward motion of the space ship was to be realized by the reaction of solid projectiles or gases leaving the nozzles with great velocity (Figure 12).[11] This is

MIECZYSLAW WOLFKE

Fig. 11

probably the first written statement of a Polish author on the application of the reaction principle to cosmic flight. (We should mention in this context the name of K. E. Tsiolkowsky [1857-1935] the founder of scientific astronautics, and a great Russian and Soviet scientist of Polish-Russian origin [his father was a Pole, his mother, a Russian woman]. Tsiolkowsky confirmed this himself in a letter to the Polish astronomer, Professor T. Banachiewicz), (Figures 13, 14).

The idea of using charged particles (namely, electrons) as the jet mass in a space rocket was formulated by A. F. Uliński,[12] probably for the first time in the scientific literature (Figure 15). The source of electron energy was the energy radiated from the sun and converted in thermoelectrical batteries to supply the voltage of about 250 kV. Uliński, a Pole living in Austria, studied rocket problems after 1913 while



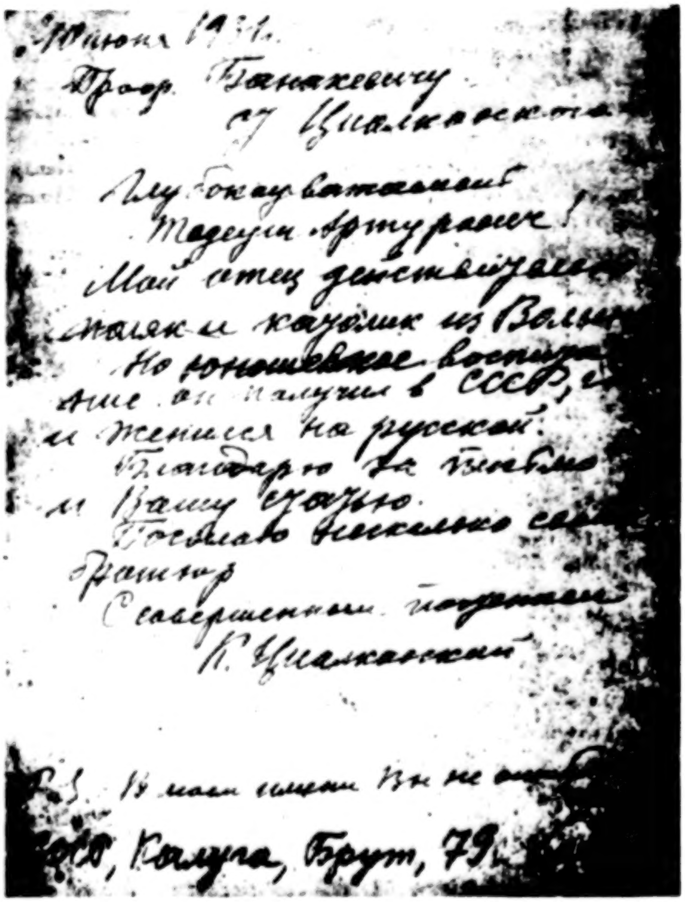
Fragment „rozprawy" o planetolocie

Fragment „rozprawy" o planetolocie

Fig. 12

serving in the Austrian army. The principles of rocket propulsion and the problems of space flight were also described by F. Burdecki (1929)[13] in what was probably the first Polish scientific-popular book on space flight.

Now we shall consider some technical and scientific papers published in the 20th century in Poland. In 1931-32, W. Vorbrodt[14] published some papers on the military applications of rockets, on flight to the Moon, and out of the Earth's gravitational field. Z. Krzywoblocki, between 1934-39, also published some theoretical papers on the military applications of rockets and the rocket propulsion of aircraft. Together with H. Stankiewicz and others, he performed some tests with rocket engines, aiming at stable flight of a winged rocket. G. Mokrzy [illegible] 5), in a popular book,[15] discussed the application of rockets to the purpose of communications on Earth and to space flight beyond the Earth. Of special interest are the theoretical papers of T. Olpiński, P. Demiańczuk and K. Zarankiewicz, dealing with the efficiency of propulsion, dynamics and the mechanics of flight, ballistics of rocket projectiles, and applications of rockets to space flight. K. Zarankiewicz[16] analysed the vertical flight of rockets in



10 июня 1931 г.
Проф. Банахевичу
от Циолковск
Глубокоуважаемый
Тадеуш Артурович!
Мой отец действительно
поляк и католик из Волы
Но юношеское воспит
ание он получил в СССР, и
и женился на русской.
Благодарю за письмо
и Вашу статью.
Посылаю несколько св
брошюр
С совершенным почтением
К. Циолковский

P.S. В моем имени Вы не ош
СССР, Калуга, Брут, 79.

10 czerwca 1931 r.

Prof. Banachiewiczowi
od Ciołkowskiego

Wielce Szanowny
Tadeuszu Arturowiczu!

Ojciec mój rzeczywiście był Polakiem i katolikiem z Wołynia. Jednak od młodości przebywał w ZSRR, gdzie ożenił się z Rosjanką.

Dziękuję bardzo za list i Wasz artykuł. Posyłam kilka moich broszur.

Z głębokim poważaniem

K. Ciołkowski

P. S. W imieniu moim nie popełniliście omyłki.
ZSRR, Kaługa, Brut 79 (adres)

Fig. 13

Fig. 14

an Earth gravitational field, in vacuum, and in the atmosphere, taking the empirical formula for the relation of atmosphere drag to different velocities. He calculated the time necessary to reach the proper parabolic velocity as a function of the accepted model of the atmosphere, the shape of the rocket, the burnout velocity of the gases, and the variation of the rockets mass (Figures 16, 17). The paper presents the exact mathematical solution of the problem.

In 1933 Oderfeld, Sachs, and Bernadzikiewicz[14] set a model jet propulsion engine in operation. Systematic work on the development of rockets was possible after

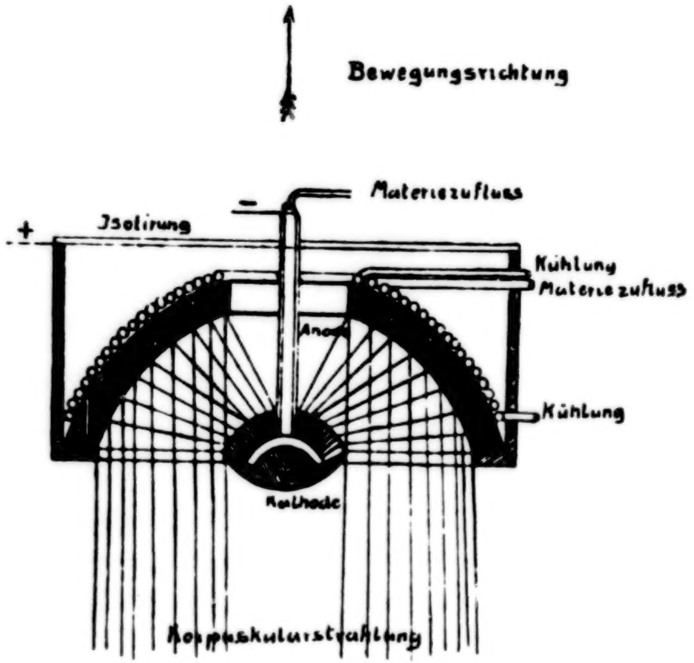
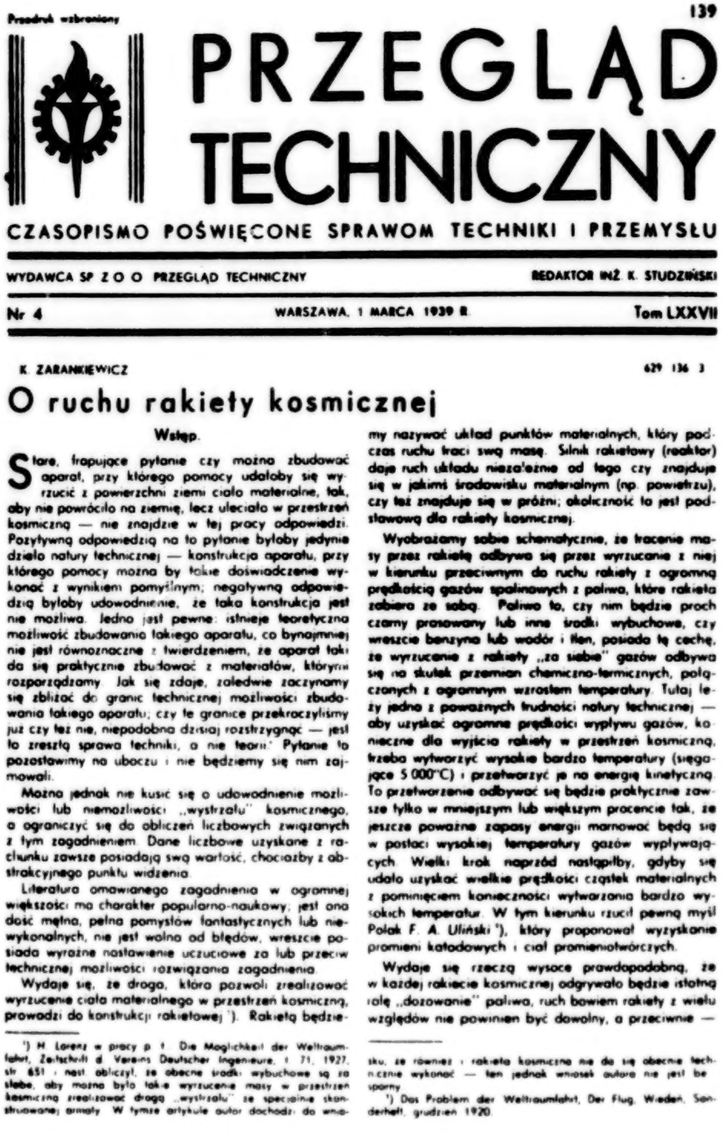


Fig. 15



Przedruk wzbroniony 139

PRZEGLĄD TECHNICZNY

CZASOPISMO POŚWIĘCONE SPRAWOM TECHNIKI I PRZEMYSŁU

WYDAWCA SP. Z O. O. PRZEGLĄD TECHNICZNY REDAKTOR INŻ. K. STUDZIŃSKI

Nr 4 WARSZAWA, 1 MARCA 1939 R. Tom LXXVII

K. ZARANKIEWICZ 629. 136. 3

O ruchu rakiety kosmicznej

Wstęp.

Stare, frapujące pytanie czy można zbudować aparat, przy którego pomocy udałoby się wyrzucić z powierzchni ziemi ciało materialne, tak, aby nie powróciło na ziemię, lecz uleciało w przestrzeń kosmiczną — nie znajdzie w tej pracy odpowiedzi. Pozytywną odpowiedzią na to pytanie byłoby jedynie dzieło natury technicznej — konstrukcja aparatu, przy którego pomocy można by takie doświadczenie wykonać z wynikiem pomyślnym; negatywną odpowiedzią byłoby udowodnienie, że taka konstrukcja jest nie możliwa. Jedno jest pewne: istnieje teoretyczna możliwość zbudowania takiego aparatu, co bynajmniej nie jest równoznaczne z twierdzeniem, że aparat taki da się praktycznie zbudować z materiałów, którymi rozporządzamy. Jak się zdaje, zaledwie zaczynamy się zbliżać do granic technicznej możliwości zbudowania takiego aparatu; czy te granice przekroczyliśmy już czy też nie, niepodobna dzisiaj rozstrzygnąć — jest to zresztą sprawa techniki, a nie teorii. Pytanie to pozostawimy na uboczu i nie będziemy się nim zajmowali.

Można jednak nie kusić się o udowodnienie możliwości lub niemożliwości „wystrzału" kosmicznego, a ograniczyć się do obliczeń liczbowych związanych z tym zagadnieniem. Dane liczbowe uzyskane z rachunku zawsze posiadają swą wartość, chociażby z abstrakcyjnego punktu widzenia.

Literatura omawianego zagadnienia w ogromnej większości ma charakter popularno-naukowy; jest ona dość mętna, pełna pomysłów fantastycznych lub niewykonalnych, nie jest wolna od błędów, wreszcie posiada wyraźne nastawienie uczuciowe za lub przeciw technicznej możliwości rozwiązania zagadnienia.

Wydaje się, że droga, która pozwoli zrealizować wyrzucenie ciała materialnego w przestrzeń kosmiczną, prowadzi do konstrukcji rakietowej 1). Rakietą będziemy nazywać układ punktów materialnych, który podczas ruchu traci swą masę. Silnik rakietowy (reaktor) daje ruch układu niezależnie od tego czy znajduje się w jakimś środowisku materialnym (np. powietrzu), czy też znajduje się w próżni; okoliczność ta jest podstawową dla rakiety kosmicznej.

Wyobrażamy sobie schematycznie, że tracenie masy przez rakietę odbywa się przez wyrzucanie z niej w kierunku przeciwnym do ruchu rakiety z ogromną prędkością gazów spalinowych z paliwa, które rakieta zabiera ze sobą. Paliwo to, czy nim będzie proch czarny prasowany lub inne środki wybuchowe, czy wreszcie benzyna lub wodór i tlen, posiada tę cechę, że wyrzucanie z rakiety „za siebie" gazów odbywa się na skutek przemian chemiczno-termicznych, połączonych z ogromnym wzrostem temperatury. Tutaj leży jedna z poważnych trudności natury technicznej — aby uzyskać ogromne prędkości wypływu gazów, konieczne dla wyjścia rakiety w przestrzeń kosmiczną, trzeba wytworzyć wysokie bardzo temperatury (sięgające 5 000°C) i przetworzyć je na energię kinetyczną. To przetworzenie odbywać się będzie praktycznie zawsze tylko w mniejszym lub większym procencie tak, że jeszcze poważne zapasy energii marnować będą się w postaci wysokiej temperatury gazów wypływających. Wielki krok naprzód nastąpiłby, gdyby się udało uzyskać wielkie prędkości cząstek materialnych z pominięciem konieczności wytwarzania bardzo wysokich temperatur. W tym kierunku rzucił pewną myśl Polak F. A. Uliński 2), który proponował wyzyskanie promieni katodowych i ciał promieniotwórczych.

Wydaje się rzeczą wysoce prawdopodobną, że w każdej rakiecie kosmicznej odgrywało będzie istotną rolę „dozowanie" paliwa, ruch bowiem rakiety z wielu względów nie powinien być dowolny, a przeciwnie —

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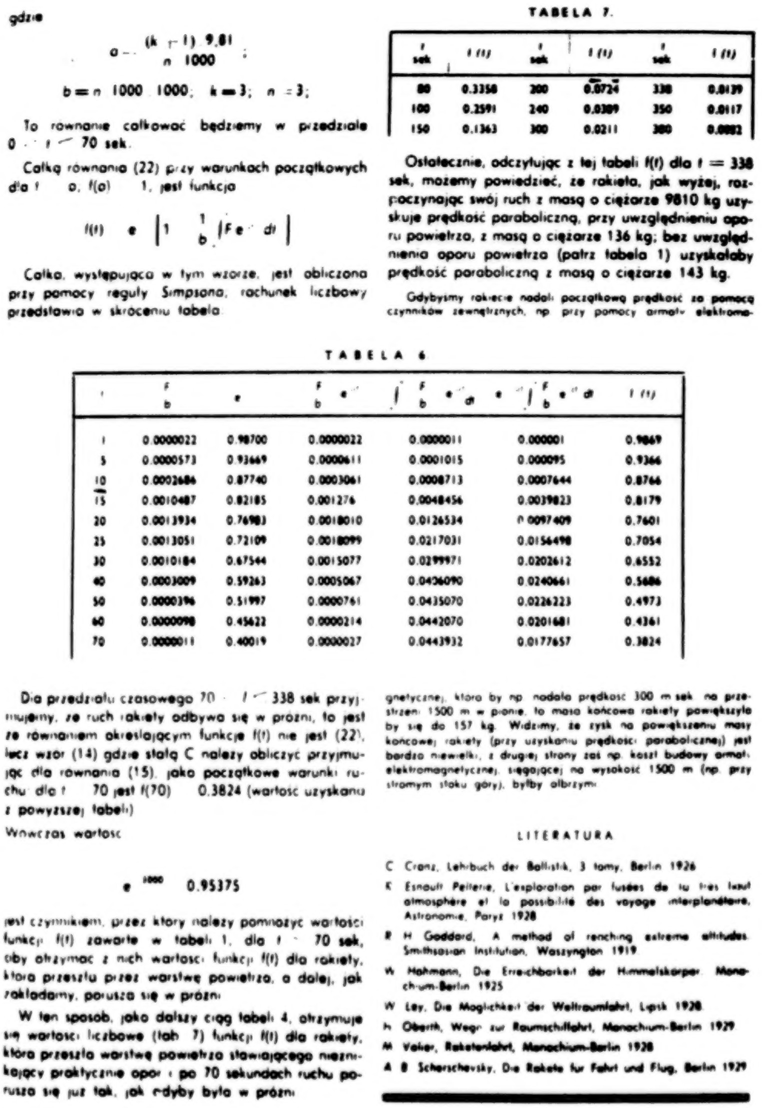
1) H. Lorenz w pracy p. t. Die Möglichkeit der Weltraumfahrt, Zeitschrift d. Vereins Deutscher Ingenieure, t. 71, 1927, str. 651 i nast. obliczył, że obecne środki wybuchowe są za słabe, aby można było takie wyrzucenie masy w przestrzeń kosmiczną zrealizować drogą „wystrzału" ze specjalnie skonstruowanej armaty. W tymże artykule autor dochodzi do wniosku, że również i rakieta kosmiczna nie da się obecnie technicznie wykonać — ten jednak wniosek autora nie jest bezsporny.

2) Das Problem der Weltraumfahrt, Der Flug, Wiedeń, Sonderheft, grudzień 1920.

Fig. 16



gdzie

$$a = \frac{(k+1)\,9{,}81}{n\,1000};$$

$$b = n\,1000\;1000;\quad k = 3;\quad n = 3;$$

To równanie całkować będziemy w przedziale $0 < t < 70$ sek.

Całką równania (22) przy warunkach początkowych dla $t = 0$, $f(0) = 1$, jest funkcja

$$f(t) = e^{-at}\left[1 - \frac{1}{b}\int F e^{at}\,dt\right]$$

Całka, występująca w tym wzorze, jest obliczona przy pomocy reguły Simpsona, rachunek liczbowy przedstawia w skróceniu tabela.

TABELA 6

t	$\frac{F}{b}$	e^{-at}	$\frac{F}{b}e^{at}$	$\int_0^t \frac{F}{b}e^{at}\,dt$	$e^{-at}\int_0^t \frac{F}{b}e^{at}\,dt$	$f(t)$
1	0.0000022	0.98700	0.0000022	0.0000011	0.000001	0.9869
5	0.0000573	0.93669	0.0000611	0.0001015	0.000095	0.9366
10	0.0002686	0.87740	0.0003061	0.0008713	0.0007644	0.8766
15	0.0010487	0.82185	0.001276	0.0048456	0.0039823	0.8179
20	0.0013934	0.76983	0.0018010	0.0126534	0.0097409	0.7601
25	0.0013051	0.72109	0.0018099	0.0217031	0.0156498	0.7054
30	0.0010184	0.67544	0.0015077	0.0299971	0.0202612	0.6552
40	0.0003009	0.59263	0.0005067	0.0406090	0.0240661	0.5686
50	0.0000396	0.51997	0.0000761	0.0435070	0.0226223	0.4973
60	0.0000098	0.45622	0.0000214	0.0442070	0.0201681	0.4361
70	0.0000011	0.40019	0.0000027	0.0443932	0.0177657	0.3824

Dla przedziału czasowego $70 < t < 338$ sek przyjmujemy, że ruch rakiety odbywa się w próżni, to jest że równaniem określającym funkcję $f(t)$ nie jest (22), lecz wzór (14) gdzie stałą C należy obliczyć przyjmując dla równania (15), jako początkowe warunki ruchu dla $t = 70$ jest $f(70) = 0.3824$ (wartość uzyskaną z powyższej tabeli)

Wówczas wartość

$$e^{1000}\quad 0.95375$$

jest czynnikiem, przez który należy pomnożyć wartości funkcji $f(t)$ zawarte w tabeli 1, dla $t > 70$ sek, aby otrzymać z nich wartości funkcji $f(t)$ dla rakiety, która przeszła przez warstwę powietrza, a dalej, jak zakładamy, porusza się w próżni

W ten sposób, jako dalszy ciąg tabeli 4, otrzymuje się wartości liczbowe (tab. 7) funkcji $f(t)$ dla rakiety, która przeszła warstwę powietrza stawiającego nieznikający praktycznie opór i po 70 sekundach ruchu porusza się już tak, jak gdyby była w próżni

TABELA 7.

t sek	$f(t)$	t sek	$f(t)$	t sek	$f(t)$
80	0.3358	200	0.0724	338	0.0139
100	0.2591	240	0.0389	350	0.0117
150	0.1363	300	0.0211	380	0.0082

Ostatecznie, odczytując z tej tabeli $f(t)$ dla $t = 338$ sek, możemy powiedzieć, że rakieta, jak wyżej, rozpoczynając swój ruch z masą o ciężarze 9810 kg uzyskuje prędkość paraboliczną, przy uwzględnieniu oporu powietrza, z masą o ciężarze 136 kg; bez uwzględnienia oporu powietrza (patrz tabela 1) uzyskałaby prędkość paraboliczną z masą o ciężarze 143 kg.

Gdybyśmy rakiecie nadali początkową prędkość za pomocą czynników zewnętrznych, np. przy pomocy armaty elektromagnetycznej, która by np. nadała prędkość 300 m/sek na przestrzeni 1500 m w pionie, to masa końcowa rakiety powiększyła by się do 157 kg. Widzimy, że zysk na powiększeniu masy końcowej rakiety (przy uzyskaniu prędkości parabolicznej) jest bardzo niewielki, z drugiej strony zaś np. koszt budowy armaty elektromagnetycznej, sięgającej na wysokość 1500 m (np. przy stromym stoku góry), byłby olbrzymi.

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Fig. 17

building a rocket test stand at the Warsaw Technical University, begun in 1938 under direction of T. Felsztyn together with A. Kowalczewski, Z. Paczkowski, D. Smoleński, W. Stolarek and Z. Tulodziecki.[14] The contributions of A. Szternfeld, born in Sieradz (1905), also must be counted among the achievements in Polish space research. He studied at the Jagiellonian University in Cracow, and later in Nancy in France. The main parts of his well known book Introduction to Cosmonautics[17] were written in France (1929-32) and in Poland (1932-1933). A. Szternfeld moved to the Soviet Union in 1935 where he continued his successful scientific work in astronautics. His book[17] was published in an extended version in the USSR in 1937.

We should also pay a tribute to the Polish underground movement during World War II that informed the Allies in England about German work on rocket development in Peenemünde on the Szczecin-Bay, and on the V-2 rocket flying tests in Middle Poland. One fired rocket was recovered by Polish partisans and sent without damage to England for careful scientific and technological investigation. Some parts of the rocket were also investigated in occupied Warsaw by Polish scientists and engineers.

FROM 1945 TO THE PRESENT

After World War II several countries pursued the development of rocketry, and scientific and practical applications were achieved in communications, meteorology, military, and the exploration of the Solar System. Many investigations were also performed in Poland.

At the beginning we shall mention some of the workers and papers on rocket technology. Propellants were investigated by L. Heger, W. Bołdaniuk and S. Ciborowski;[14] liquid propellants by S. Wójcicki; ballistic properties of solid propellants were investigated by D. Smoleński, M. Zembrzuski, M. Dybek, W. Kowalczyk, J. Deweryniak, E. Woźniak, M. Parulska, J. Grzegorzewski and J. Jórczak. Thermodynamics and rocket internal ballistics were the subjects of the work of Z. Paczkowski (in a monograph Rocket Flight Mechanics), D. Marchel, F. Wolnica and R. Odolinski; stability of the flight of rockets with stabilizers Z. Paczkowski and L. Wasilewska; measurements in rocket technique, K. Kowalewski, J. Pakleza, W. Styburski and M. Zembrzuski; rocket guidance, S. Sławiński, R. Dmowski, A. Lizoń, K. Holejko, E. Olearczuk, Z. Katliński and S. Paszkowski; some military rocket applications were developed by S. Wojciechowski, A. Arciuch, W. Kozakiewicz, J. Wiśniewski, S. Zukowski, Z. Paczkowski, S. Paszkowski, T. Burakowski, A. Sala, S. Hornung et al.[14]

In the Rocket and Satellite Research Department of the Polish Hydrological-Meteorological Institute (PIHM), measurements are conducted on the direction and velocity of the wind at different heights, and the temperature of the troposphere and stratosphere. Pictures are received of cloud fields transmitted by meteorological satellites as well as pictures in the infrared part of the spectrum. Atmosphere sounding is performed with the meteorological rockets "Meteor-1, 2, and 3." These investigations were begun by J. Walczewski, G. Pawlak and J. Kibinski.[14] O. Wolczek and M. Subotowicz have worked on the theory of the nuclear, ion, and plasma rocket engines. M. Bielecki, W. Dudonis, R. Janiczek, Z. Kotliński, B. Korner, K. Makowski, E. Olearczuk,

Z. Paczkowski, M. Subotowicz, K. Zarankiewicz, W. Zakowski, et. al, have concentrated on the theory of the rocket flight. In many papers, M. Subotowicz investigated the theories of classic and relativistic multistep rockets. He also generalized the well-known formula of Ackeret, of the velocity of the one step relativistic rocket on the case of the multistep relativistic rocket.[18] Different rocket parameters, and the general characteristics of the rockets and spaceships, were investigated in the papers of M. Subotowicz, R. Szymański, R. Vogt and S. Wójcicki.

The popularization of rocket and astronautical problems has been performed by E. Bialoborski, K. Borun, Z. Brodzki, J. Gadomski, W. Geisler, A. Marks, J. Salabun, M. Subotowicz, O. Wołczek, K. Zarankiewicz, et al. The following people are dealing with the history of rocket and space research in Poland: W. Geisler, E. Olszewski, T. Przypkowski, T. Nowak, M. Subotowicz, J. Thor, et. al.

The possibility of using new rocket techniques for space research stimulated Polish scientists. At the beginning, this work was concentrated in the Astronautical Department of the Polish Academy of Sciences (PAN), directed by K. Zarankiewicz (1955-59). In 1960 the monograph of M. Subotowicz Astronautyka[18] was published, covering progress in astronautics up to 1960. In 1964 the Committee of the Research and Peaceful Use of the Space was established in PAN; rocket and satellite investigations of the Sun were realized under J. Mergentaler, and of the interaction of primary cosmic rays with matter under M. Miesowicz. Also, a regular service was organized to observe artificial satellites of the Earth.

The investigations of J. Gadomski[19] on the ecospheres of the stars at distances of about several light years around the Sun has become widely known. The ecosphere around a particular star means the "life zone," where the thermal conditions exist near the planets, enabling macro-molecules to develop of essential biological importance in the development of living organisms. It follows from the analysis of J. Godomski that at a radius of about 17 light years around the Sun there exist 14 stars of solar spectral class that possess ecospheres. But there are only three stars of astronautical interest.

An interesting project was suggested by O. Wołczek[20] to investigate the cosmologic processes in space by exploding hydrogen bombs beyond the Earth's atmosphere. Different problems of space physics were investigated in the papers of W. Fiszdon and J. Jatczak (cosmic ray physics), K. Kordylewski (the matter of the libration points of the Earth-Moon system), and A. Januszajtis, M. Lunc, A. Marks, M. Subotowicz, O. Wolczek, and J. Walczewski. Some suggestions concerning the possible verification of the general theory of relativity using astronautical methods were presented by M. Subotowicz. This same author presented some proposals to detect antimatter in a Moon laboratory.

The references concerning all the papers mentioned in this section may be found either partly in reference 14, or in the scientific and popular astronautical journals published in Poland: Technika Rakietowa (1957-63), Postepy Astronautyki (from 1967), Technika Lotnicza i Astronautyczna, Biuletyn Informacyjny of the Polish Astronautical Society (1956-57), and Astronautyka (from 1958). We have not considered here the papers on space medicine directed by Kaulbersz, Bilski, J. Walawski, S. Barański, Z. Jethon, Z. Kaleta, or papers on space law directed by M. Lachs, J. Machowski, J. Sztucki and others. All these papers should be referred to separately.

In Poland, the Polskie Towarzystwo Astronautyczne (Polish Astronautical Society), founded on December 30, 1954, and Komitet d/s Badań i Pokojowego Wykorzystania Przestrzeni Kosmicznej Polskiej Akademii Nauk (Committee on Space Research and its Peaceful Use of the Polish Academy of Sciences), deal with problems concerning scientific and popular work in astronautices. The Polish Astronautical Society (PTA) has about 800 members and publishes the scientific quarterly Postepy Astronautyki, and the scientific-popular bimonthly Astronautyka. Every two years PTA organizes Scientific Conferences on the Rocket Technique and Astronautics, and represents Poland during the International Astronautical Federation (IAF) Congresses and in the Commissions of the IAF and the International Academy of Astronautics in Paris. PTA also sponsors the scientific research in astronautics and popularizes astronautical achievements. In 1976 the Polish Academy of Sciences established the research in close cooperation with the socialist countries program "Intercosmos," including manned and unmanned flights around the Earth, and to the Moon and planets.

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THE U.S. ARMY AIR CORPS JET PROPULSION RESEARCH PROJECT, GALCIT PROJECT No. 1, 1939-1946: A MEMOIR[+]

F. J. Malina (USA)[++]

I. INTRODUCTION

This memoir is a sequel to the one I wrote on the GALCIT (Guggenheim Aeronautical Laboratory, California Institute of Technology) Rocket Research Project 1936-38, for the First International Symposium on the History of Astronautics, organized by the International Academy of Astronautics at Belgrade on 25-26 September, 1967.[1] As I pointed out then, I fully recognize the fallibility of memory and the unavoidable injection of personal evaluations and judgements.

Whereas few written records for the period 1936-38 of rocket research at the California Institute of Technology (Caltech) remain, during the period 1939-46, numerous formal reports were prepared under contracts to agencies of the U.S. government, and are available to anyone interested. On the other hand, during the latter period our research work became secret, so that there are not many personal records of an intimate kind to turn to for aspects of developments that frequently are more interesting than cold, formal reports. It is a misfortune that minutes of the weekly research conferences held between 1939 and April 1944 are no longer to be found in the archives of the Jet Propulsion Laboratory (JPL).[+++] Secret classification of research also prohibited the free publication of results between 1940 and 1946. For this reason, some of these results are still not as well known as the more highly publicized activities of other groups during this period in the USA and in other countries, especially in Nazi Germany. This situation was aggravated

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[+] Presented at Third History Symposium of the International Academy of Astronautics, Mar del Plata, Argentina, October 1969.

[++] Co-Founder and Director (1944-1946) of the Jet Propulsion Laboratory, California Institute of Technology. Trustee-Past President, International Academy of Astronautics.

[+++] Although the designation JPL was used for the first time in 1944, the work of JPL is considered to include rocket research at Caltech initiated by the Galcit Rocket Research Group from 1936 onwards.

by the fact that several key persons who led the research at JPL dispersed after the end of the Second World War. Summaries of various aspects of this work, some published, can be found in References 2 to 10.

In September 1939, Nazi Germany invaded Poland and World War II began. This had a direct impact upon the rocket research plans of the GALCIT Rocket Research Group. Work toward our dreams of designing rockets for scientific research at high altitudes and for space flight had to be deferred for several years. We had anticipated the outbreak of war in Europe some time before it began, and our thoughts turned towards the use of rocket propulsion as an auxiliary to the propeller-piston engine power plants then in general use for aircraft. We had been authoritatively told by a senior officer of the U.S. Army Ordnance Department that there was little possibility of applying rocket propulsion in military missiles.[4]

Inter-service rivalry over rockets would appear several years later. As late as 1944, the Army Air Corps, by then called the Army Air Forces, readily allowed the Jet Propulsion Laboratory to undertake America's first research program on long range rocket missiles for the Army Ordnance Department. At the time, Army Air Forces foresaw little possibility of such missiles replacing many functions of bomber aircraft in warfare. It is surprising that Allied military intelligence had no inkling of the advanced state of military rocket development in Germany until 1943.

When General Henry H. Arnold, Chief of the Army Air Corps, visited Caltech[8, 9] in May 1938, Theodore von Kármán (1881-1963) (Figure 1) first learned that interest was developing in the use of rocket propulsion for military aircraft. I prepared a report in August for the Consolidated Aircraft Company (now called General Dynamics/Convair) at San Diego, California, on the possibility of using rocket propulsion for assisting the take-off of large aircraft, especially flying boats[11] (Figure 2). In early December, after giving a talk entitled "Facts and Fancies of Rockets" at a Caltech luncheon of the Society of the Sigma Xi, I was informed by von Kármán, Robert A. Millikan (1868-1953), and Max M. Mason, that I was to go to Washington, D.C., to give expert information on rocket propulsion to the National Academy of Sciences Committee on Army Air Corps Research. Mason was chairman and R. A. Millikan and von Kármán were members of the Committee.

General Arnold had asked the Academy for advice on a number of subjects, one of which was the possible use of rockets for the assisted take-off of heavily loaded aircraft. Some feared that sufficiently long runways would be unavailable in combat areas. (Later, others feared that the new jet engines would have low power on take-off and would require very long runways.) Actually, the bulldozer solved the problem on land, making long runways practical. Rocket-assisted take-off of aircraft on aircraft carriers, however, soon assumed importance to the U.S. Navy.

I prepared a study entitled "Report on Jet Propulsion for the National Academy of Sciences Committee on Air Corps Research," which contained the following parts:

Fig. 1
Theodore von Kármán in 1939 (circa)

(1) Fundamental concepts, (2) classification of types of jet propulsors, (3) possible applications of jet propulsion in connection with heavier-than-air craft, (4) present state of development of jet propulsion, and (5) a proposed research program for developing jet propulsion.[12] The word "rocket" was still in such bad repute in "serious" scientific circles in the USA at this time that von Kármán and I felt it advisable to follow the precedent of the Air Corps by dropping the use of the word. It did not return to our vocabulary until several years later, when the word "jet" had become part of the name of our laboratory (JPL), and of the Aerojet General Corporation.

I presented my report to the National Academy's Committee on December 28, 1938, and shortly thereafter the Academy accepted von Karman's proposal for a study by our GALCIT research group of the problem of the assisted take-off of aircraft as well as the preparation of a detailed plan for an extensive research program. The Academy provided a sum of $1,000 for this study, which was to be completed in about six months. Incidentally, when Caltech obtained this first government grant for rocket research, Jerome C. Hunsaker of the Massachusetts Institute of Technology agreed to study for the Air Corps the deicing problem of windshields, then a serious aircraft problem, and told von Kármán: "You can have the Buck Roger's job."[9]

Fig. 2
Frank J. Malina in 1943 (circa)

John W. Parsons (1914-1952) (Figure 3) and Edward S. Forman were delighted when I returned from Washington with the news that our efforts during the previous three years were to be rewarded by financial support from the government, and that von Kármán would devote more of his time to the work. We could even expect to be paid for doing rocket research. Parsons, Forman and I were the only members of the original GALCIT research group at this time still carrying on at Caltech. We proceeded to collect more information on rockets for assisting the take-off of aircraft, and to accumulate experimental data on rocket motor performance with solid propellant rockets and with the gaseous propellant engines in a test stand we had constructed the previous year.

In May 1939, as part of his survey of aircraft development for the Army Air Corps, Charles A. Lindberg came to Caltech after visiting Robert H. Goddard at his New Mexico research station. Since von Kármán was away on one of his rather frequent trips, Clark B. Millikan (1903-1966) briefed Lindberg on aeronautical research at GALCIT, and I told him about our studies of the possible use of rocket propulsion for aircraft. He said

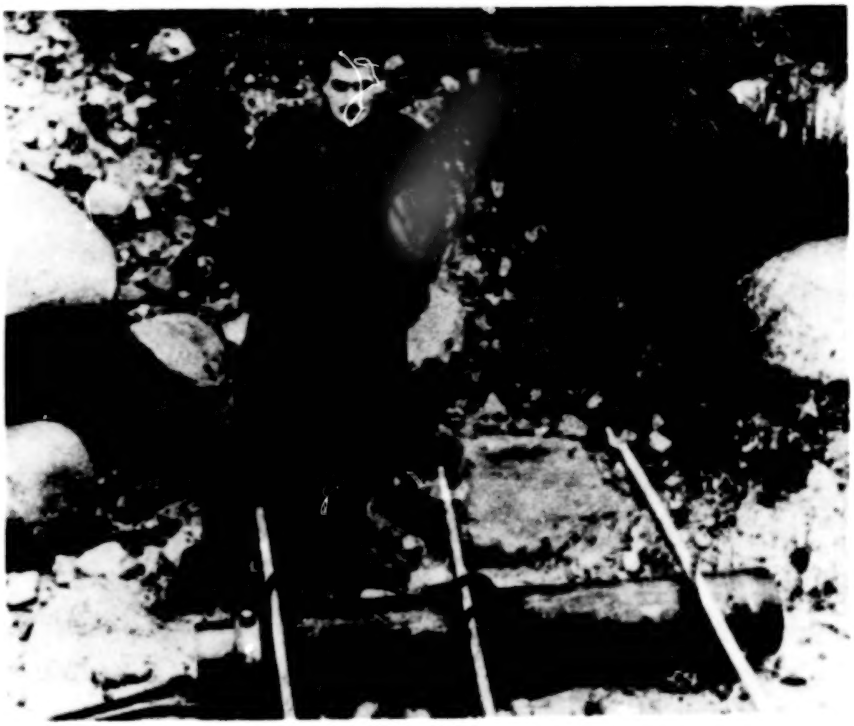


Fig. 3
John W. Parsons in 1940

nothing about his visit with Goddard.[4] It is odd that none of the military services, to my knowledge, ever requested JPL to send copies of reports to Goddard, although we had a considerable mailing list of individuals and organizations. Similarly, none of Goddard's reports to governmental agencies were ever received at JPL.

About this time I learned that Eugen Sänger was carrying on rocket research in Germany in reply to a letter I had sent him in Vienna.[+] But information on rocket research in Nazi Germany that began in the early 1930's was unavailable to the Project until November 1943, when we received British intelligence reports on the V-2 missile work at Peenemunde. No information on rocket research in the USSR was available during the period of this memoir. Later, in September 1944, I went to England on a mission for the Ordnance Department where I obtained detailed information on British rocket research. I obtained further information during a second mission in the autumn of 1946. Some reports had been received by the project on British work beginning in 1940, and several British researchers visited us during the following years.

[+] See Irene Sänger-Bredt, "Memoir: The Silver Bird Story and the Development of the Aerospace Transporter," in this volume - Ed.

The studies and experiments we carried out in the spring of 1939 made us sufficiently confident of the possibility of developing both solid and liquid propellent rocket engines to the extent that we prepared a proposal to the National Academy for a $100,000 program of research and facilities construction for the fiscal year 1939-40, beginning on July 1, 1939. Von Kármán took the proposal to Washington only to find that our optimism was not shared either by the National Academy or by the Air Corps. In his autobiography, von Kármán recounts that while discussing the proposal with Major Benjamin Chidlaw (later Commanding General of the Air Material Command) he was asked "do you honestly believe that the Air Corps should spend as much as $10,000 for such a thing as rockets?" This amount turned out to be the maximum that could be obtained. It meant that our experimental work would have to be done either on the campus of Caltech, where our presence was not very popular, or with temporary portable setups in the Arroyo Seco river bed above Devil's Gate Dam on the western edge of Pasadena.

The contract, sponsored by the National Academy of Sciences, came into force on 1 July 1939, bringing into being the Army Air Corps Jet Propulsion Research Project. (A year later the Army Air Corps took over direct sponsorship of the Project.) Under its terms, studies were to be made of a number of basic problems connected with the development of rocket engines for application to the "super-performance" of aircraft. The term "super-performance" was defined to include: (a) shortening of the time and distance required to takeoff, (b) temporary increase of rate of climb, and (c) temporary increase of level flight speed. The contract also wisely authorized work to be done on both liquid- and solid-propellant rocket engines.

Von Kármán, then 58 years of age, became actively committed to the development of rocket propulsion by assuming direction of the Project. Parsons, Forman, and myself as chief engineer, formed the nucleus of the staff. He brought to our work his vast experience of utilizing mathematics and fundamental physical principles for the solution of difficult engineering problems, and a rare skill in negotiation and organization. Parsons was then 25; Forman and I were 27. While von Kármán was away, I chose the designation GALCIT Project No. 1 for the Air Corps research. When he returned he surprised me by frowning at the designation. He said I evidently did not know what House No. 1 meant in China. At the Air Material Command, Wright Field, Dayton, Ohio, the Project was known by the designation Aircraft Laboratory Project MX 121.

We carried out the experimental work partly on the campus of Caltech and partly in the Arroyo Seco above Devil's Gate Dam in Pasadena during the first year of the Project. In 1940, six acres on the western bank of the Arroyo Seco were leased from the Water Department of the City of Pasadena for the duration of World War II. Approximately 40 acres had been leased from the City by 1946 and this area is still a part of the tract on which the Jet Propulsion Laboratory is located. Most of the temporary structures for offices and testing have disappeared, since then replaced by permanent installations. Residents near the Project put up with the noise of rocket testing until the end of the war in 1946 but, thereafter, noisy experiments were shifted to other installations, for example, in the Mojave desert. Facilities of the Project in 1941, in 1945, and in 1969 are shown in Figures 4 (a) and (b), 5 and 6.

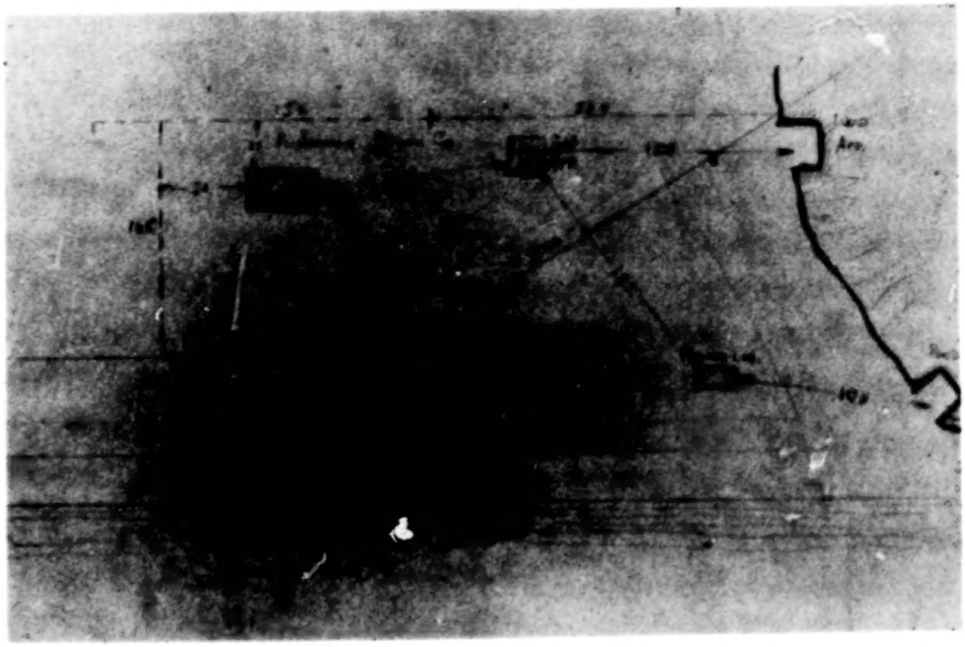


Fig. 4 (a)
Sketch in 1940 of Layout of First Project
Facilities to be Constructed at the
Arroyo Seco, Pasadena Site

The Project benefited greatly from the use of special Caltech laboratory equipment, and for advice from members of the faculty and staff. For example, Aladar Hollander, Linus A. Pauling, and Fritz Zwicky were frequently consulted. A Chemistry Group under the direction of Bruce H. Sage began working on chemical problems of propellants for the project in 1942. Also, several Caltech staff members served as senior research engineers for the Project on a part-time basis. The fact that Zwicky became one of our consultants in 1940 had ironic overtones. While working on the theory of rocket propulsion for my doctoral thesis in 1937,[13] I mentioned to him some difficulties I was having in my study. He exploded with the opinion that I was wasting my time on an impossible subject. For, he said, I must realize that a rocket could not operate in space as it required the atmosphere to push against to provide thrust! By 1940 he realized that he was mistaken.

It is not possible in this memoir to mention the many devoted men and women who carried out rocket research, assisted in the construction and testing of experimental

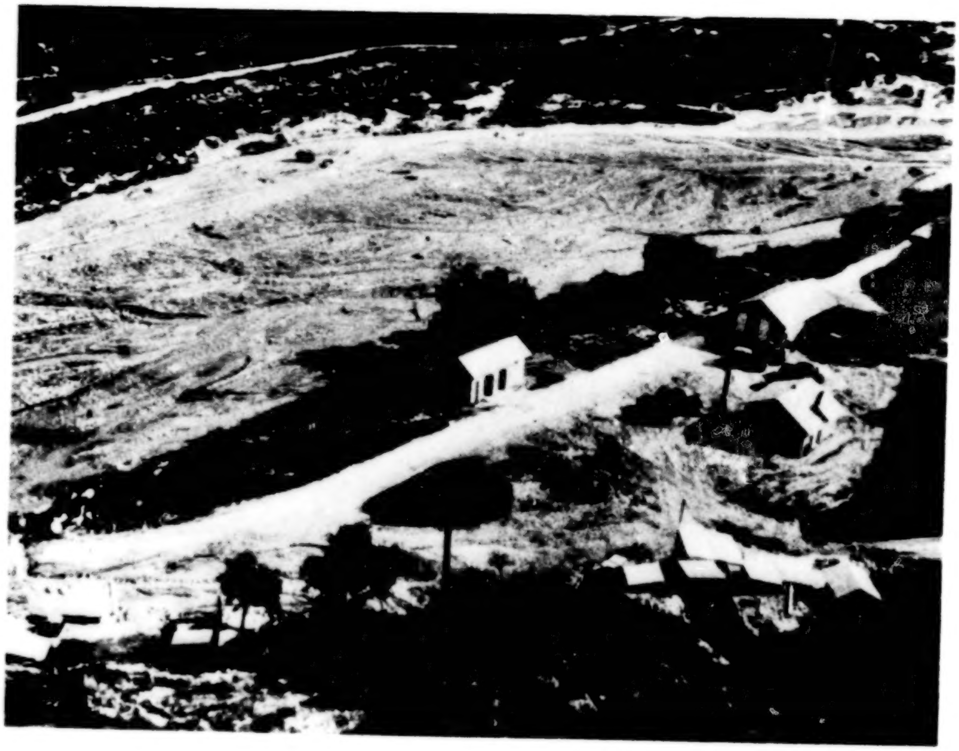


Fig. 4 (b)
View of Project Facilities in 1941

devices, made designs and computations, and helped with the administration of the Project. E. S. Forman and E. M. Pierce, Sr., were key persons in the first phase of the installation of buildings and facilities. Pierce, who was loved by all, was my administrative mainstay during the period covered by this memoir. It was no easy matter, in the midst of World War II, to assemble a qualified staff that grew in number each year. Obtaining scarce materials and equipment was a constant, frustrating trial. A group photograph of the Project personnel in 1945 is shown in Figure 7.

II. FUNDAMENTAL STUDIES OF ROCKET MOTORS

Characteristics and Performance Parameters of a Rocket Motor

H. S. Tsien and I began theoretical studies of the characteristics of an ideal rocket motor consisting of a chamber of fixed volume and an exhaust nozzle in 1936. The results of these studies up to the end of 1939 are given in References 13 and 14. I developed a universal ideal-thrust diagram showing the dependence of thrust on the expansion ratio of the exhaust nozzle, the ratio of chamber pressure to external pressure, the

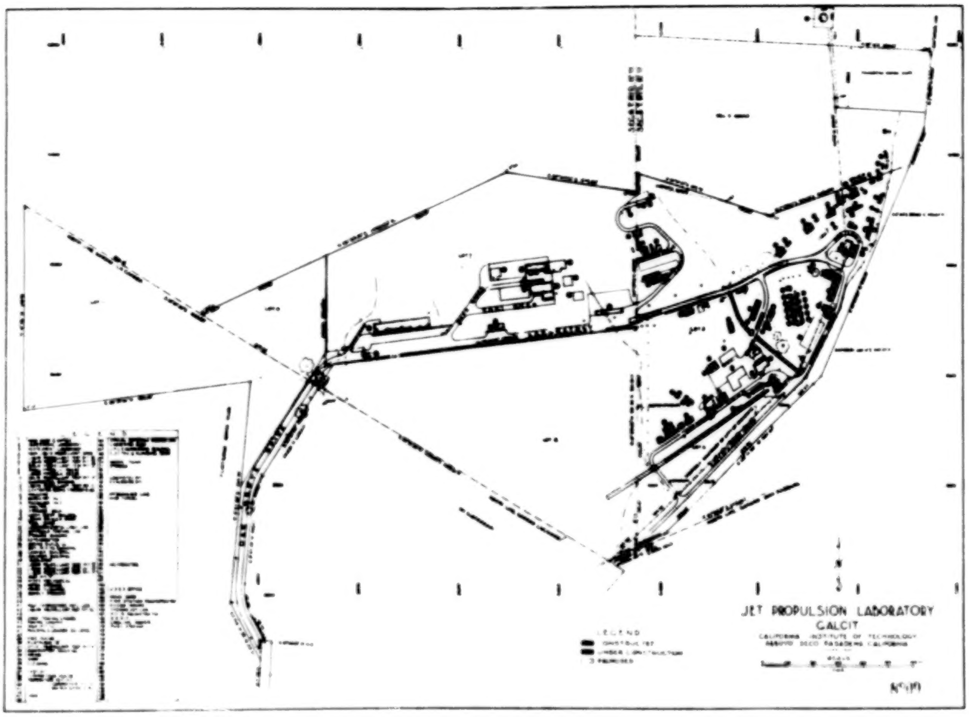

JET PROPULSION LABORATORY
GALCIT
CALIFORNIA INSTITUTE OF TECHNOLOGY
ARROYO SECO PASADENA CALIFORNIA
LEGEND
N

Fig. 5
Layout of the Facilities of the Jet Propulsion Laboratory, GALCIT in June 1945

exit angle of the exhaust nozzle and the specific heat ratio of the exhaust gas (Figure 8). A form of this diagram is now used for determining the ideal thrust coefficient, C_F, of a rocket motor. The effect of the angle of divergence of the exhaust nozzle on thrust under ideal conditions was calculated by Tsien.

Experimental studies of the characteristics of a rocket motor were carried out, beginning in 1938, by Parsons, Forman and myself with the gaseous propellants, oxygen and ethylene.[1] Data were first obtained with oxygen alone to check the test stand installation and to compare results with those reported by Bartocci in March 1938. These were followed by data obtained with the combustion of oxygen and ethylene.[13, 15] During one of the first series of tests with this combination, in March 1939, the oxygen line exploded, scattering parts of the apparatus over a large area. Though shaken, Parsons and Forman, who were conducting the test, were unhurt. A piece of the Bourdon tube of one of the pressure gauges buried itself in a wooden beam about where my head would have been if von Kármán had not called me away earlier.

Martin Summerfield joined the Project in July 1940 and continued these studies with an improved test installation (Figure 9). In particular, he determined the value of

Fig. 6
Aerial View of the Jet Propulsion Laboratory
in 1965

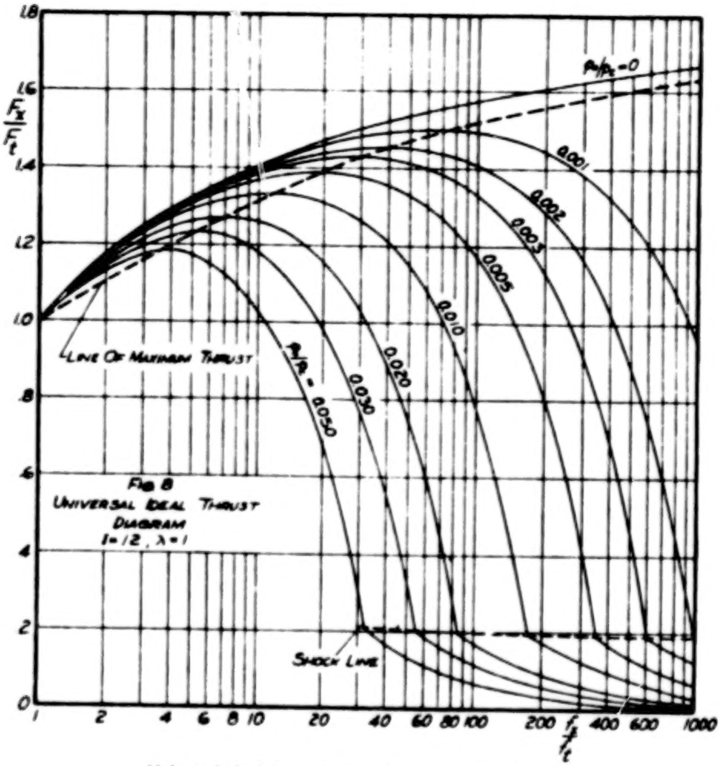
the thrust coefficient under real conditions, as it was effected by the angle of divergence of the exhaust nozzle,[2] and he found that thrust augmentors gave little promise from a practical point of view.[5] He also investigated the significance of the ratio of the combustion chamber volume to the nozzle throat area, L*, proposed by Sänger for determining the required propellant stay-time in a combustion chamber. It is connected with chemical kinetics and affects combustion efficiency.[5]

While conducting experiments with liquid propellants in 1941, Walter B. Powell, following a discussion with Mark M. Mills, introduced a useful parameter called the characteristic velocity, c^*, which is defined so as to give the effective exhaust velocity, c, as a product of the experimental coefficients, C_F AND c^*. The characteristic velocity is determined only by the properties of the propellant and the nozzle throat area. Thus, it is independent of exit conditions and may be considered as the parameter indicating the efficacy of the combustion process.[2,5] At about this time another useful parameter, called the specific impulse, I_{sp}, came into use. It is ordinarily expressed in pounds thrust per pound of propellant consumed per second.

Fig. 7
Personnel of the Jet Propulsion Laboratory,
GALCIT in 1945 (circa)

Stability of Restricted Burning Solid Propellant Rocket Units

One of our first objectives was to develop a solid propellant rocket unit capable of delivering a constant thrust on the order of 1000 pounds for a period of 10 to 30 seconds. As far as was known, no black powder or smokeless power rocket had ever been constructed to meet these specifications of thrust and duration. Experts we consulted were very dubious about the possibility of doing so. Preliminary experiments made by Parsons and Forman with pressed solid propellant charges restricted to burn cigarette-fashion appeared to support this view.[15] It was generally believed that the combustion chamber pressure of a restricted burning solid rocket unit would continue to rise from the moment of ignition until any combustion chamber of reasonable weight would burst. In other words, it was thought that such combustion was inherently unstable. Von Kármán, in the spring of 1940, after listening both to the opinions of the experts and to the explosions of Parson's rockets, one evening at his home wrote down four differential equations describing the operation of an ideal restricted burning motor, and asked me to solve them (Figure 12). Much to our relief we found that, theoretically, a restricted burning unit would maintain a constant chamber pressure as long as the ratio of the area of the throat of the exhaust nozzle to the burning area of the propellant charge remained constant,



Universal ideal thrust diagram for $\gamma = 1.2$ and $\lambda = 1$.

Fig. 8
Universal Ideal Thrust Diagram (cf. Ref. 14)

that is, the process is stable.[16,17] Experimental verification of the theory was soon obtained. (Cf. Section III). (It has been shown that the theory is correct provided the chamber frequency is low [that is L* is large]).

Cooling of Rocket Motors

A rocket motor operates under more severe conditions of high temperature and of continuous rate of heat release than any other heat engine utilizing chemical combustion. For these reasons, problems of heat transfer are among the most acute and important in rocket motor design. Rocket engines for the assisted take-off of aircraft require motors to operate for a maximum period of up to around 30 seconds. Tests made by various experimenters in the 1930s showed that for such a duration is was not necessary to cool the walls of the combustion chamber and exhaust nozzle. In an uncooled motor, thermal equilibrium in the materials of construction is not reached during the safe period of operation; if it were to be reached, the motor would become a molten mass. Work carried out

Fig. 9
Martin Summerfield and Edward G. Crofut in
Front of Gas Propellant Rocket
Test Stand in 1941

by the Project on the design and construction of uncooled solid propellant units and of liquid propellant motors is discussed in Sections IV and V below. The uncooled liquid propellant motor of 1999 lb. thrust used in the A-20A flight tests in 1942 had a safe operating period of 75 seconds, but it weighed 90 lbs.[18,19]

High performance, long duration rocket motors require the use of refractory liners or cooling of the walls in the case of liquid propellant motors by all or part of the propellants. In the 1930s, researchers demonstrated the feasibility of constructing regeneratively cooled motors in which the coolant liquid absorbs heat as it circulates around the motor in ducts and is then injected into the combustion chamber. Extensive studies of regeneratively cooled motors were begun in 1942 by Summerfield and Seifert.[2,20] When they began these studies, practically no information was available on the various aspects of regenerative cooling that would permit the design of a motor to meet the Air Corps performance specifications for various applications. There were, in fact, doubts

$$\text{I.}\quad m_{\hat{b}} = \Gamma f_{\hat{t}} \frac{p_{\hat{c}}}{\sqrt{R T_{\hat{c}}}} + \frac{d m_{\hat{c}}}{dt}$$

$$\text{II.}\quad m_{\hat{b}} = \frac{\rho_{\hat{p}} R T_{\hat{c}}}{p_{\hat{c}}} \left(\frac{d m_{\hat{c}}}{dt} - \frac{m_{\hat{c}}}{p_{\hat{c}}} \frac{d p_{\hat{c}}}{dt} + \frac{m_{\hat{c}}}{T_{\hat{c}}} \frac{d T_{\hat{c}}}{dt} \right)$$

$$\text{III.}\quad m_{\hat{b}} = \frac{m_{\hat{c}}}{T_{\hat{a}} - T_{\hat{c}}} \frac{d T_{\hat{c}}}{dt}$$

$$\text{IV.}\quad m_{\hat{b}} = (k + k_{\hat{1}} p_{\hat{c}}) f_{\hat{c}} \rho_{\hat{p}}$$

where

$f_{\hat{c}}$ = burning area of propellant
$f_{\hat{t}}$ = area of exhaust nozzle throat
k = propellant rate of burning coefficient
$k_{\hat{1}}$ = propellant rate of burning coefficient
$m_{\hat{b}}$ = mass of propellant burned per second
$m_{\hat{c}}$ = mass of gas in combustion chamber
$p_{\hat{c}}$ = combustion chamber pressure
R = universal gas constant
t = time, seconds
$T_{\hat{a}}$ = maximum possible combustion temperature
$T_{\hat{c}}$ = temperature of gases in combustion chamber
γ = ratio of specific heats of gas
$\Gamma = \left(\frac{2}{\gamma+1}\right)^{\gamma+1/2(\gamma-1)} \sqrt{\gamma}$
$\rho_{\hat{p}}$ = mass density of propellant

Fig. 10
Four Differential Equations That Describe the
Operation of an Ideal Solid
Propellant Rocket Motor

cast on the principle of regenerative cooling for motors operating at higher values of specific impulse because it appeared to be a "boot-strap" process. But theoretical and experimental studies showed that the principle was sound[9] and data were accumulated to permit the design of such motors.

Search for Materials

The high gas temperatures and velocities encountered in rocket motors and the unusual characteristics of chemicals used as liquid propellants posed special problems whose solution could not be found in other domains of heat-engine technology. Systematic studies of materials were begun by the Project in 1942, including the properties of steel, aluminum and magnesium alloys, ceramics and materials produced by means of powder metallurgy. It is comforting to note that humans, in cooperation with nature, provided the materials required by the designers of various types of rocket engines. The trials and tribulations of those that searched for materials are evident in the early reports by N. Kaplan and R. J. Andrus and by Mills,[21,22] in the monthly reports and in the conference minutes of the Project.[23, 24]

III. ROCKET PROPELLANTS

Liquid Propellants

When the development of a liquid propellant rocket unit for use aboard aircraft was discussed with the Air Corps, we decided that the project should attempt to use aviation gasoline as a fuel and something besides liquid oxygen (LOX) as an oxidizer. Liquid oxygen, the ideal oxidizer from a rocket performance point of view, had been used by Goddard, members of the American Rocket Society, and others. However, the problems of producing, transporting and storing LOX in 1939 (or at any time, as far as the military services were concerned) were considered so formidable that it should be avoided. In today's idiom, the Air Corps wanted rocket engines that utilized "storable propellants."

Parsons, in his report of June 1937,[25] suggested, among other storable oxidizers, a mixture of nitric acid and nitrogen pentoxyde. In 1939, he recommended the choice of red fuming nitric acid, a solution of nitric acid and nitrogen dioxyde, hereafter called RFNA. This oxidizer has poisonous properties and is very corrosive, requiring the use of stainless steel or aluminum to contain it. Nevertheless, it was more acceptable to the Air Corps than LOX. Just before Christmas, 1939, tests in an open crucible showed that RFNA would burn with gasoline and benzene. As pointed out in Section V, Summerfield and Powell subsequently found in testing actual rocket motors that RFNA and gasoline led to unstable combustion. The resulting pulses in some cases became so great that the combustion chamber exploded. The phenomenon of "throbbing" has not been completely cured to the present day.

A Chemistry Group, directed by Sage, was set up at Caltech at the beginning of 1941 to investigate the RFNA-gasoline reaction and the properties of other possible liquid propellants. We began to dream of the advantages of a fuel that would be spontaneously ignitible with RFNA. It would dispense with the need of an ignition system and might burn more satisfactorily with the oxidizer. In early February 1942, I visited the rocket research group at the Naval Engineering Experiment Station at Annapolis, Maryland, directed by an old friend, Lt. Robert C. Traux. While discussing the problem of RFNA-gasoline combustion with Ensign Ray C. Stiff, the chemical engineer of the group, I learned that he had found in the chemistry literature a reference to the property of aniline to ignite spontaneously with nitric acid. He wondered if it would be of any help to add aniline to gasoline.

During the overnight train trip from Annapolis to Dayton, Ohio, it occurred to me that we should try replacing gasoline entirely with aniline as a fuel. This would complicate Air Corps logistic problems, but it seemed it might be the necessary price to pay for an engine that would not explode unpredictably. Upon arriving in Dayton, I sent a telegram to Summerfield asking him to try the idea. When I returned to Pasadena a few days later, he greeted me with exultation. We had a reliable, storable, liquid propellant rocket engine!

Parsons and I filed a patent on 8 May 1943 for a reaction motor operable by liquid propellants and the method of operating it--a motor using spontaneously ignitable propellants.[26] Long after the war, I learned that Lutz and his collaborators in Germany had stumbled on what they called hypergolic propellants at about the same time. It is interesting to note that our patent included, among other suggestions, the use of hydrazine as a fuel with nitrogen dioxide. This is the basic combination used in the engines constructed by the Aerojet General Corporation for the Apollo Service Module, and by the Bell Aircraft Corporation for the Apollo Lunar Excursion Module, which so far have performed without fail in the flights of men to the Moon. The project initiated research on hydrazine and its compounds in 1945.

We encountered considerable resistance from the military services to the acceptance of the toxic aniline as a replacement for gasoline. The Air Material Command finally gave way when it became evident that the A-20A flight tests, scheduled to start within two months, could not be made without risk of catastrophe if gasoline was used as a fuel. The Navy Bureau of Aeronautics continued to resist the use of aniline by the Annapolis group for almost another year, when a violent explosion that wrecked their nitric acid-gasoline test stand made them accept it.

After completing the successful flight tests of the A-20A aircraft equipped with two uncooled 1000 lb. thrust, 25 seconds, RFNA-analine engines were completed (cf. Section VI), the project initiated detailed studies of the problems of propellant injection into the rocket motor, a search for other spontaneously ignitable propellant combinations--such as furfuryl alcohol and nitric acid not containing nitrogen dioxide or "white acid"--and of methods of cooling long-duration motors and of supplying propellants by means of gas pressure and of pumps (cf. Section V). Work was initiated on the development of engines utilizing liquid oxygen in October 1942.[23, 24, 27] The outlook of the project on liquid propellants in 1943 is described in Reference 28.

In 1944 studies of monopropellants, such as nitromethane and hydrogen peroxide, began. In 1937 Parsons already had listed tetranitromethane as a possible rocket propellant. The advantage of a monopropellant would be a much simpler engine, since only one propellant tank, one pump and one control valve would be required. Although nitromethane is comparatively easy to handle, it is sensitive to temperature, has a tendency to explode under impact or shock, and is difficult to ignite and sustain a reaction in a combustion chamber.[29] To the best of my knowledge, a satisfactory rocket engine utilizing nitromethane has not been developed.

Hydrogen peroxide, although it has found a place in presentday rocket technology, was regarded with fear and suspicion by the Project. This attitude arose when 60 pounds of H_2O_2 in a stainless steel tank exploded in the summer of 1944. The cause was not definitely determined; however, the summer atmospheric temperature of about 100°F and the possibility of foreign material in the tank were felt to have been contributing factors.[23]

Solid Propellants

Although there have been centuries of experience with black powder rockets, and several investigators used smokeless powker and Ballistite in rockets between about 1918 and 1939, none of these rockets had the thrust and duration required for the aircraft "superperformance" applications. Parsons and Forman in 1938 built and tested a smokeless powder constant-volume combustion motor similar to the one that had been used by Goddard.[1] We concluded after these tests that the mechanical complications of constructing an engine using successive impulses to obtain thrust durations of over 10 seconds was impractical. Upon Parsons' recommendation, we concentrated our efforts on the development of a motor provided with a restricted burning powder charge that would burn at one end only at constant pressure to provide a constant thrust.

Parsons started with the traditional sky rocket. This type of pyrotechnic device was propelled by a black powder charge pressed into a cardboard combustion chamber with a conical hole in its center. The gases escaped through a rounded clay orifice. Its efficiency was very, very low, but it was reliable. The conical hole in the charge was believed to be the secret that kept the charge from burning down the sides of the container to produce chamber pressures that would burst the container. The longest duration of thrust of this motor did not exceed about 1 second.

During 1939 and 1940, various mixtures based on black powder and mixtures of black powder with smokeless powder were tested in 1 in. and 3 in. diameter chambers. The charge for the 3 in. chamber was made up of 6 in. long pellets compressed at around 6,500 p.s.i. that were coated with various substances to form a solid or liquid seal between the charge and the walls of the chamber (Figure 3). The charge of the 1 in. chamber was pressed directly into the chamber in small increments at pressures between 7,700 and 12,000 p.s.i. Most of the tests of these charges ended in an explosion. Mechanical causes for failures, such as burning of the charge on the surface next to the wall because of leakage, transfer of heat down the walls sufficient to ignite the sides of the charge, and cracking of the charge under combustion pressure, were suspected. However, there were those who were convinced that the combustion process of a restricted burning charge in a rocket motor was basically unstable. It was only when the von Kármán-Malina analysis of the characteristics of the ideal solid propellant rocket motor was made in the spring of 1940 (Section II), that proved the process was stable, that a concentrated effort was made to study the mechanical causes of failure.[15, 16]

Hundreds of tests were then made with different powder mixtures, using black powder as the basic ingredient, with various loading techniques and with various motor designs. The dependence of chamber pressure on the ratio of chamber cross section area to nozzle throat area was determined for each specific powder mixture.

By the spring of 1941 results were sufficiently encouraging to schedule flight tests of an aircraft equipped with solid propellant rockets specially designed for it. (A discussion of the flight tests of the Ercoupe airplane is given in Section VI.)[30] The propellant charge used in the Ercoupe motors was a type of amide black powder designated as GALCIT 27. The 2 lb. charge was pressed into the combustion chamber, which had a blotting paper liner, in 22 increments by a plunger with a conical nose shape at a pressure of 18 tons. The diameter of the charge was 1.75 in. and its length varied between 10 and 11 in. The motor was designed to deliver about 28 lb. thrust for about 12 seconds (Figures 11(a) and 11(b)). Eighteen rocket motors were delivered every other day for the flight tests at March Field, California, about an hour's drive from the Project. During the first phase of the flight tests one motor failed explosively in a static test and one while the Ercoupe was in level flight. Thereafter, 152 motors were used in succession without explosive failure. The motors were prepared by Parsons, Forman, and Fred Miller.[30]

It was most fortunate that the flight tests were carried out close to the location of the Project, which permitted the rocket motors to be fired within a few days from the time they were charged with propellant. Following the flight tests, it was found that after the motors were exposed to simulated storage and temperature conditions over several days they exploded in most cases. It was evident that either the blotting paper liner or the mechanical characteristics of the propellant were unsatisfactory. But the Navy Department regarded the successful Ercoupe tests with much interest from the point of view of application of rockets for the assisted take-off of aircraft from aircraft carriers. Upon the urging of Lt. C. F. Fischer of the Bureau of Aeronautics, who had witnessed the tests, a contract was placed by the Navy with the Project in early 1942 for the development of a 200 lb. thrust, 8 second unit. The unit was designated by the acronym JATO for Jet Assisted Take-Off, and this designation is still used.

This Navy contract came in the midst of the explosive failures of the JATO unit developed for the Ercoupe tests. All efforts to improve the amide-black powder propellant and loading techniques of the motor developed for the Ercoupe tests failed to meet specified storage conditions ranging from Alaska to Africa. Investigations of motors using

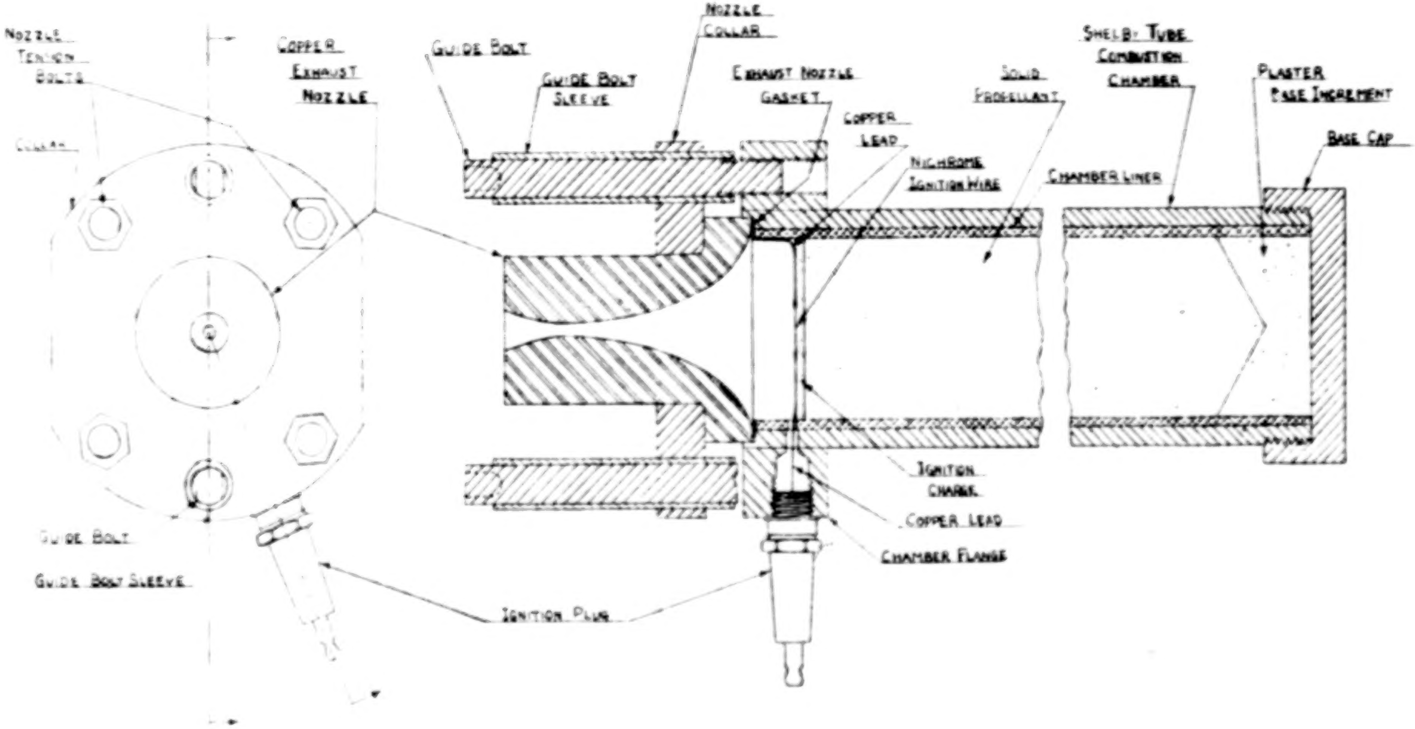

NOZZLE TENSION BOLTS
COLLAR
COPPER EXHAUST NOZZLE
GUIDE BOLT
GUIDE BOLT SLEEVE
NOZZLE COLLAR
EXHAUST NOZZLE GASKET
COPPER LEAD
NICHROME IGNITION WIRE
SOLID PROPELLANT
CHAMBER LINER
SHELBY TUBE COMBUSTION CHAMBER
PLASTER BASE INCREMENT
BASE CAP
IGNITION CHARGE
COPPER LEAD
CHAMBER FLANGE
GUIDE BOLT
GUIDE BOLT SLEEVE
IGNITION PLUG

Fig. 11 (a)
Drawing of the 25 Lb. Thrust, 12 Second
Solid-Propellant JATO Unit Used In
The Ercoupe Flight Tests,
August 1941

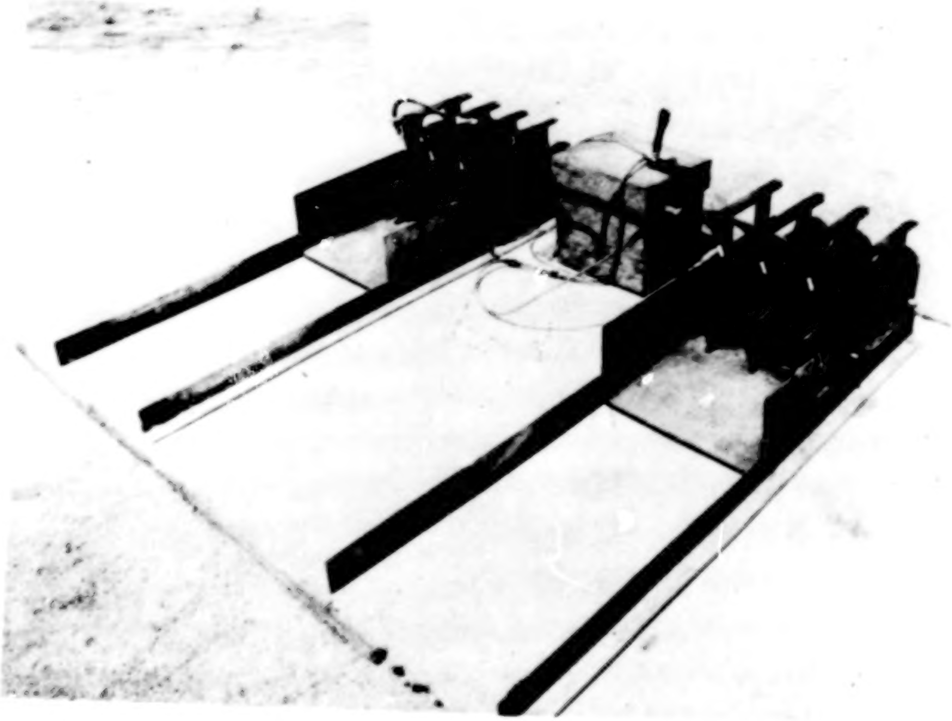


Fig. 11 (b)
View of Six JATO Units, Attachment Assemblies
and Ignition System for Ercoupe Flight Tests

Ballistite, a compound essentially of nitrocellulose and nitroglycerine, also proved negative, mainly because of its ambient temperature sensitivity, that is, the variation of its rate of burning with ambient temperature. For example, a JATO unit designed to deliver 1,000 lb thrust at 90°F would deliver at most only 600 lb. thrust at 40°F. Though the duration of thrust at the lower temperature would be lengthened, an aircraft assisted under such a condition might fail to take off from a short runway.

Thus, the spring of 1942 was one of desperation for those concerned with development of a reliable solid propellant JATO unit. We know that theoretically it was possible to construct such an engine, but no one came forward with a promising idea until June, when Parsons, no doubt after communing with his poetic spirits, suggested trying a radical new propellant. It would consist of potassium perchlorate, as oxidizer, common asphalt used on roads as a binder and fuel, and be cast, after being mixed, into a combustion chamber. A test of the propellant, designated GALCIT 53, was quickly made and the results were so promising that work on other propellant types was dropped for a long time. Parsons was assisted in the development of the asphalt base propellant by Mills and Fred Miller.[31] After due study of the origin of the ideas for the new propellant, Parsons was recognized as its inventor and a patent was granted in his name.

At first, the Ordnance Department objected strongly to our use of potassium perchlorate as an oxidizer because it had proved unsafe in the past. Parsons realized that their objection was no longer valid, since ways had been found to produce the material with a minimum purity of 99%. Impurities in the form of dangerous chlorates had been practically eliminated. The ruling of the Ordnance Department was thereafter changed, allowing the use of this kind of solid oxidizer. The Navy contract for 100 JATO units delivering 200 lb. thrust for 8 seconds was successfully completed, with GALCIT 53 as the propellant.[32] (Figure 12) Production of service-type units for the Navy began shortly thereafter at the Aerojet Engineering Corporation (now Aerojet General Corporation, cf. Section VIII).

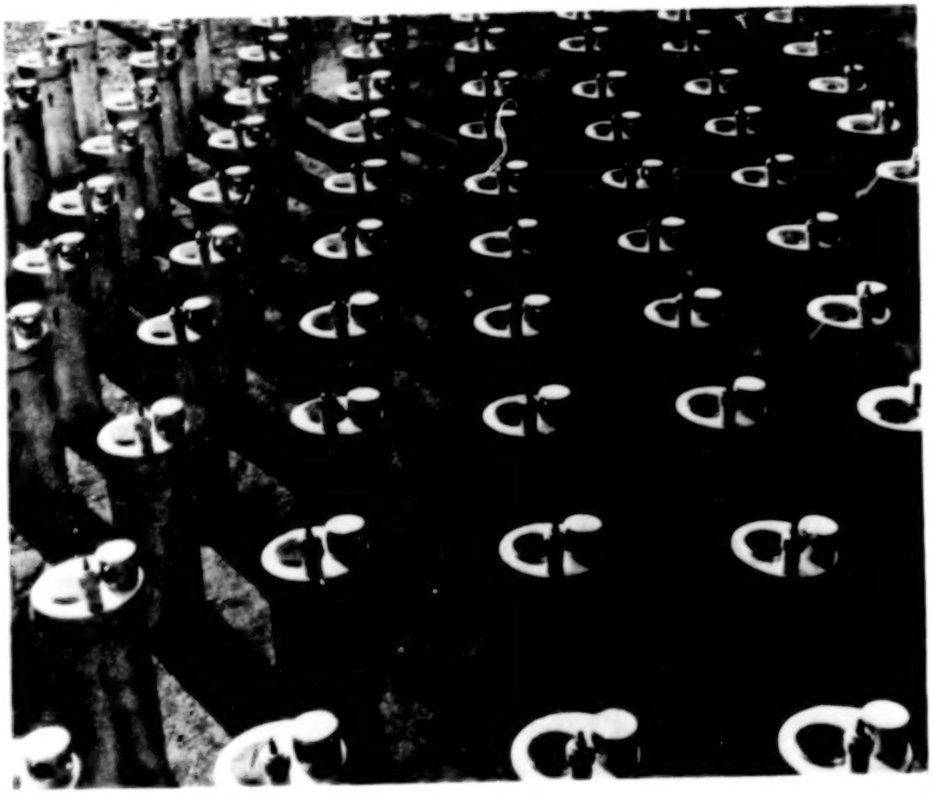


Fig. 12
View of 200 Lb. Thrust, 8 Second Solid-Propellant
JATO's Produced in 1942 For the U.S. Navy

The project carried out extensive studies on asphalt-base propellants (composite propellants, as they are now called) in the following years. A detailed report released in May 1944 on the propellant GALCIT 61-C by Mills can be found in Reference 32. GALCIT 61-C consisted of 76% potassium perchlorate and 24% fuel. The fuel component was 70%

Texaco No. 18 asphalt and 30% Union Oil Company Pure Penn SAE No. 10 lubricating oil. The fuel was liquified at about 275°F, the pulverized potassium perchlorate added to it, and the mixture thoroughly stirred. The mixture was then poured into the combustion chamber, which had been previously lined with a material similar to the fuel component, and allowed to cool and become hard. This propellant when burned at a chamber pressure of 2,000 p.s.i. had a chamber temperature of 3,000-3,500°F, a specific impulse of 186, and an exhaust velocity of about 5,900 ft. per sec. Storage temperature limits were from -9°F to 120°F. It was developed in 1943 and used in service JATO units by the Navy until the end of World War II.

Solid propellants utilizing potassium perchlorate as oxidizer produce dense clouds of white smoke (potassium chloride), which the Navy did not like at all. Some months after GALCIT 53 was developed, Parsons informed the Project weekly research conference that he had eliminated the smoke problem by replacing potassium perchlorate with ammonium perchlorate. Navy rocket experts were immediately invited to visit the Project for a demonstration. When they arrived we posted ourselves some distance from the test pit, the red flag was run up, and Parsons gave the order for his latest creation to be fired. We beheld a big cloud of white smoke and Parsons with a look of surprise on his face. He sheepishly explained that the smoke must have been caused by the humidity, for the air had been very dry on the days they had made tests before. Ammonium perchlorate does reduce the amount of smoke produced if the air is dry, but it produces undesirable chloride in the jet.

The project also studied the possible use of other fuels instead of asphalt, such as Napalm gelled hydrocarbon, gelled wax mixtures, and butyl rubber. A continuation of studies of the last material later led Charles Bartley, under the JPL-ORDCIT Project in 1945, to the discovery of the advantages of the castable elastomeric (polysulfide rubber) material called Thiokol. This discovery became the basis of solid propellant manufacture by the Thiokol Chemical Corporation. Air Force Material Command terminated work by the Project on solid propellant motors on June 30, 1944; the Ordnance Department continued the work for long range missile applications.

In concluding this part on the work of the Project in developing solid propellants for long duration rocket motors, it should be pointed out that two of the most important discoveries in the long history of solid rockets were made here. First it was theoretically proved by the Kármán-Malina analysis that a stable constant pressure, long duration solid propellant rocket motor was possible. Second, Parsons found a new kind of material, the asphalt-potassium perchlorate composite solid propellant, which initiated modern castable solid propellant rocket technology. The 200 lb. thrust JATO unit has grown into solid propellant rocket engines today delivering on the order of 1,000,000 lb. thrust.

IV. SOLID PROPELLANT MOTOR DESIGN

A solid propellant rocket motor consists of a combustion chamber containing the propellant charge, an ignitor, and an exhaust nozzle through which the combustion products escape to give thrust. In the first motor design made by the Project, the nozzle was attached to the combustion chamber by means of bolts that pulled apart at a chamber pressure considerably below the pressure that would shatter the walls of the chamber.[30] In 1942 Mills and I devised a pressure-release device or "safety plug" that considerably simplified motor design. A patent was granted to us for this device on 14 May 1946.[32,34]

Great care was taken to protect personnel involved in preparing solid propellants and in testing solid and liquid propellant engines. During the period of this memoir no serious injury was suffered by any member of the Project, in spite of the tendency of those daily working with explosives to develop a contempt for them through familiarity. Parsons was the fatal victim of this hazard in 1952 when he dropped some fulminate of mercury while moving his private laboratory from Pasadena to Mexico.[1]

The work of the Project concentrated on the design criteria for restricted burning motors that would be suitable for JATO units and, later for long range missiles. During the course of this research, engineers were provided with methods of motor component design when the following characteristics of the propellant to be used were known:

a. Sensitivity of the propellant to ambient temperature during combustion.
b. Combustion pressure limit below which the propellant burns in an irregular manner.
c. Combustion pressure limit above which the propellant burns in an unpredictable manner.
d. Storage characteristics of the propellant charge from the point of view of minimum and maximum ambient temperatures allowed and possible decomposition of the propellant with prolonged storage.
e. Ignition temperature of the propellant.
f. Rate of burning of the propellant as function of the combustion pressure.
g. Performance characteristics of the propellant to produce rocket thrust.

The great progress made in the scientific design of solid propellant rocket motors in comparison with the empirical, traditional, method used in previous centuries can be appreciated by reference to the text "Jet Propulsion"[2] prepared for the course at Caltech at the request of the Air Technical Service Command in 1943 and continued in following years (cf. Section IX). The development of the solid propellant JATO unit showed that for many applications it was superior to liquid propellant engines; because of its simplicity and reliability. The debate on the superiority of solid vs. liquid propellant rocket engines for boosters of space vehicles still rages today.

I would like to point out that it was the policy of the Project to concentrate efforts on fundamental research problems of rocket engine development, leaving pilot and service stage design problems to industry. Von Kármán and I were convinced that this was the appropriate stance to take for an educational and research institution such as Caltech. The fact that this policy was violated during later periods in the history of JPL was due to special circumstances that prevailed in the U.S.A. at that time.

V. LIQUID PROPELLANT ENGINE DESIGN

When the Project initiated work on rocket engines for the "superperformance" of aircraft in 1939, it was not evident whether either a solid or a liquid propellant type could be constructed to meet service requirements. Therefore, we investigated both types. We realized that a solid propellant JATO unit would be simpler, lighter, and more practical logistically, even though it could not be stopped and restarted. On the other hand, for auxiliary propulsion in flight, the liquid propellant engine, with its higher specific impulse, possible longer burning duration, and controllable thrust, would have great advantages. The use of such an engine for the sole propulsion of an aircraft or of a missile was not at first contemplated in the program of the project. The study I prepared in October 1939 on the application of rocket propulsion to a radio-controlled flying torpedo was used by Captain (later Rear Admiral) D. S. Fahrney (then head of the guided missile project of the Bureau of Aeronautics of the Navy), as the basis for the design and production of America's first guided liquid propellant rocket missile in the U.S.A., the GORGON.[8,34]

RFNA - Gasoline Engine Research

A liquid propellant engine consists essentially of a rocket motor, propellant feed and control system, and propellant tanks (Figure 13). Summerfield initiated experiments directed toward the design and construction of a JATO unit using RFNA and gasoline on 1 July 1940 on the basis of experience that we had gained with tests of the gaseous oxygen-methyl alcohol motor in 1936, experiments with the gaseous oxygen-ethylene motor begun in 1937, various theoretical studies we had made, and the meager information available in the published literature.

The Project's first permanent installation or test pit for experiments on liquid propellant rocket engine components was completed in February 1941. Propellant was fed to a motor by regulated nitrogen gas pressure from standard commercial tanks. Tests were begun on an uncooled motor designed to deliver 200 lb. thrust at a combustion chamber pressure of about 300 p.s.i. The exhaust nozzle, which posed no special design problems, was made in a copper block and attached to the chamber by means of bolts that

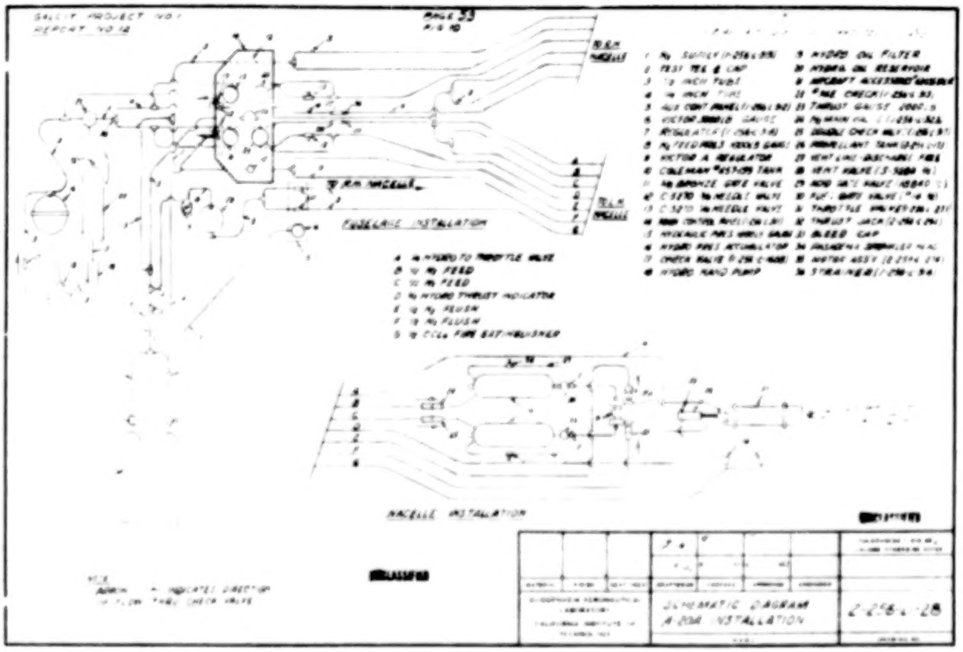

GALCIT PROJECT NO. 1
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FIG 10
R.H. NACELLE
L.H. NACELLE
TO R.H. NACELLE
FUSELAGE INSTALLATION
A 1/4 HYDRO TO THROTTLE VALVE
B 1/2 N2 FEED
C 1/2 N2 FEED
D 1/4 HYDRO THRUST INDICATOR
E 1/2 N2 FLUSH
F 1/2 N2 FLUSH
G 1/2 CCl4 FIRE EXTINGUISHER
HYDRO OIL FILTER
HYDRO OIL RESERVOIR
THRUST GAUGE
BLEED CAP
HYDRO HAND PUMP
NACELLE INSTALLATION
UNCLASSIFIED
SCHEMATIC DIAGRAM
A-20A INSTALLATION
2-256-C-28

Fig. 13
Schematic Diagram of the Liquid Propellant
(RFNA-Aniline), Gas Pressure Supply
System JATO Engine Used in A-20A
Flight Tests

would break at a pressure below the bursting pressure of the chamber. The volume and shape of the chamber and the mode of injecting RFNA and gasoline into it to obtain regular, efficient combustion had to be determined as well as the method of obtaining ignition.

The first type tested had an impinging-stream injector with four orifices in a flat plate, two for RFNA and two for gasoline, and a spark plug for ignition. Since the motor in a JATO unit would be placed in a horizontal position, it was so tested. Three motors were tried and all failed explosively. The third one, in May 1941, set fire to the railroad ties that made up the sides of the test pit and caused considerable damage to the test equipment. The test pits of the Project were deliberately built facing brush-covered hillsides in order to stop jets and flying metal. The brush, which during the long, dry Southern California season is highly flammable, was cleared near the pits. But we were constantly worried that one day the brush higher up would ignite in a big wind and that the fire would race up the mountains toward the Mt. Wilson Observatory above us. A fire did once break out, but it was stopped with the help of all hands at the Project. A sprinkler system was then installed on the hillsides and no further brush fires troubled us.

The chief cause of the liquid propellant motor failures was improper ignition. If ignition was not instantaneous, propellant accumulated in the chamber and, if it then ignited, an explosive reaction took place. It was concluded that the injector did not produce fine enough streams to obtain adequate mixing. A new injector was made with six impingement points; each was made up of a central gasoline stream and two RFNA streams from angled orifices. The spark plug was shielded to prevent short circuiting by spray. The motor with these modifications worked repeatedly without failure and led to tests of a similar, larger, motor designed to deliver 500 lb. thrust. Tests of this motor were also successful, except for one explosion which should have been a warning to us.

At this point we concluded that we knew notably the following, as reported by Summerfield and B. M. Forman (the cousin of E. S. Forman):[36]

a. How to introduce the propellants into the motor, including a satisfactory gas pressure propellant supply system and control valves.

b. The effect of RFNA-gasoline mixture ratio on combustion chamber temperature and the specific impulse of a motor.

c. How to reduce exhaust nozzle erosion by chrome plating the copper surface.

Tests on the 500 lb. thrust motor, which coincided with the successful Ercoupe flight tests at March Field (cf. Section VI), encouraged us, in September 1941, to make the following important decisions:

a. Design immediately a 1000 lb-thrust uncooled motor to operate for a minimum duration of 25 seconds.

b. Form two groups; the first to continue basic studies of engine design, and the second to begin the design of an experimental JATO unit suitable for installation on an aircraft.

c. Request the Army Air Forces (AAF) (formerly the Army Air Corps) to provide an aircraft for flight tests of the JATO unit (cf. Section VI).

Tests at the beginning of October of the first 1000 lb. thrust motor, provided with an injector with 15 impingement points and two shielded spark plugs, proved to be very disappointing. Sometimes ignition was so long delayed that a very "hard start" resulted; sometimes it failed altogether. What really disturbed us, however, was our first encounter with the unpleasant phenomenon of combustion pulsing or "throbbing" that has plagued rocket engineers for the past 30 years. Sporadically and unpredictably a motor begins to throb, slightly at first but with increasing intensity until, if not promptly shut off, it blows up.

For four months various attempts were made to overcome the phenomenon but without success. In the meantime, arrangements had been completed with the AAF for flight tests in the Spring of 1942. The JATO unit that was to be installed on the 14,000 lb. Douglas bi-motor bomber, the A-20A, was in final design stage. In early February 1942, I sent the telegram to Summerfield from Dayton, Ohio to replace gasoline with aniline as a fuel. My suggestion worked. Throbbing was eliminated or rendered negligible (cf. Section III). The "throbbing" months drew von Karman, Summerfield, and the Sage Chemistry Group into a concerted theoretical and experimental attack on the problem. Von Kármán became so fascinated with the problems of liquid propellant combustion that he pursued them until his death in 1963; Summerfield is still working on them. Von Kármán broadened the field to include aerothermodynamics, and Summerfield subsequently branched off to include the combustion process of solid propellants.

Engine Research With RFNA and Spontaneously Igniting Fuel For the A-20-A Flight Tests

The property of aniline to ignite upon contact with nitric acid greatly simplified motor design. Auxiliary ignition methods could be dispensed with, and the danger of propellant accumulating in large quantities in a horizontally mounted motor was avoided—provided the propellant components arrived simultaneously at an appropriate mixture ratio. The short ignition lag when aniline comes into contact with RFNA, compared to gasoline, greatly helped to reduce throbbing and eliminated the destructive buildup of pulses. The importance of spontaneously igniting chemicals, or "hypergolic" propellants (a German term), for rocket engines opened up an aspect of chemical research that had been given very little attention in the past and this research contributed to a better understanding of the kinetics of chemical reactions.

The liquid propellant JATO unit group, consisting of Summerfield, Powell and E. G. Crofut, incorporated the RFNA-aniline combination into a revised design. It was found that performance specifications established for the 100 lb. thrust motor using the storable propellants could be met by an injector with only four pairs of impinging streams. For the A-20A flight tests, no attempt was made to produce a lightweight unit but rather effort was concentrated on reliability and safety. The tail surfaces of the aircraft were estimated to be high enough to clear exhaust jets when the motors were mounted in the nacelle tail cones, where there was also sufficient space for the two propellant tanks and flow control valves. Standard commercial nitrogen tanks and pressure regulators were installed in the fuselage together with controls for starting and stopping the units, operable by a mechanic in the rear gunner's cockpit upon instructions from the pilot.[18, 37] A view of the installation in one of the nacelles is shown in Figure 14. The motor was mounted on slides and provided with hydraulic jacks, so that if an explosion separated the exhaust nozzle block, the remainder of the motor would not impose too great a shock on the aircraft nacelle structure.

The two JATO units performed satisfactorily during 44 successive firings on the A-20A. The propellant control valves, which were hydraulically operated, and the check

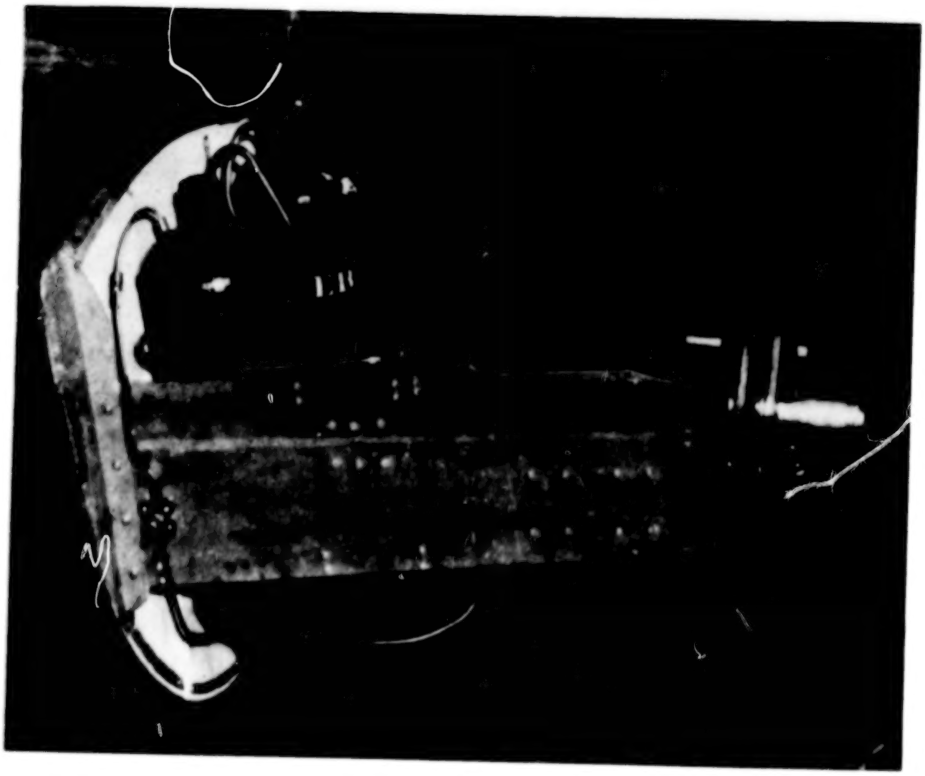

Fig. 14
View of the RFNA-Aniline JATO Engine Installed in
One of the Nacelles of A-20A Airplane

valves gave the most trouble. The check valves were removed at an early stage of ground testing. We had some malicious satisfaction when one of the conventional piston engines on the A-20A developed mechanical trouble and delayed the test program for two days. The highly successful tests of our experimental JATO units during the flight tests led the AAF to place a contract for the production of service-type units at the Aerojet Engineering Corporation--the newly formed company's first contract (cf. Section VIII). The Project, meantime, resumed basic research on rocket engine components.

Engine Research With Various Liquid Propellants After the A-20A Flight Tests

I prepared a review of developments in liquid propellant jet (rocket) propulsion at the Project and at Aerojet for a special meeting of the Office of Scientific Research and Development (OSRD) in Washington, D.C. on February 17, 1944.[38] It will be noted from the title of the report that at this time the term "rocket," in parenthesis, was used. The term was being rehabilitated in technical and military circles in the U.S.A. for the first time since the nineteenth century, when rifled guns replaced military rocket missiles. On the other hand, there was still general resistance to mentioning the use of rockets for space research and exploration, and none is made in the report, even though we were already reviving the studies we had interrupted in 1939.

Motors. A comprehensive memorandum on the state and direction of development of liquid propellant rocket motors by the Project and by other groups was prepared by Seifert and me for the May 29, 1945 meeting of the Coordinating Committee of the Guided Missile Program at the request of the Ordnance Department of the Army Service Forces.[39] (There was still no reference made to space research and exploration.)

An idea of the range of research on liquid propellant rocket motors and other engine components that was undertaken by the project at the end of the period covered in this memoir can be obtained from the following headings of Monthly Summary No. 1-54, 1 to 30 November 1946.[23] (Research on solid propellant rocket engines was taken over by the ORDCIT Project beginning on 1 July 1944):

- A. Fundamental Research
  - 1. New Propellants
    - Hydrogen peroxide
      - (i) Performance with fuels
    - Liquid oxygen
      - (i) Performance with fuels
    - Acid-hydrocarbon combinations
      - (i) WFNA-Furfuryl alcohol
  - 2. Investigation and development of new materials
    - a. Survey of the physical properties of ceramic materials
    - b. Development of ceramic materials for use as turbine blades
    - c. Preparation and properties of refractory chamber liners
    - d. Development of porous materials for sweat cooling
  - 3. Temperature measurement and heat-transfer analysis
- B. Engineering Development
  - 1. Injector and chamber designs
    - a. Effect of injectors on performance, ignition and heat transfer (with nitromethane)
  - 2. Motor-cooling techniques
    - a. Film cooling (with acid and hydrocarbons)
  - 3. Hydraulic measurements
    - a. Characteristics of rocket components
      - (i) Atomization
      - (ii) Fluid metering

The same monthly summary contained a financial report showing that out of $944,000.00 available that year for research from the AAF, an estimated $954,211,52 had been spent! I do not remember who flew to the Air Material Command, Wright Field, Dayton, Ohio, to get more money, but it was obtained. I was then preparing for my two-year leave of absence from Caltech to go to Paris to work for international scientific cooperation at UNESCO, and I was turning over responsibility for the direction of JPL to my successor, Louis G. Dunn. As I write this memoir twenty-five years later, I am still in France, where I have been occupied since 1953 with non-governmental international cooperation in astronautics and visual fine art.

Propellant Feed Systems. The following types of feed systems were studied by the Project: the gas pressure feed system using stored air or nitrogen in tanks at around 2000 p.s.i., and using gas generators; centrifugal pumps with various drives; and the Centrojet principle proposed by Aerojet.[2, 38] The system using stored gas becomes excessively heavy if thrust durations exceeding about 60 seconds are required, and if combustion pressures higher than 300 p.s.i. are desired to obtain better specific impulse. The idea of providing gas at pressure by means of a chemical reaction to replace storage tanks sounded very good and, beginning in 1942, considerable effort was made to develop it; however, a practical system was not achieved by the end of 1946.[2, 23 & 24]

In 1942, the Project began the development of high speed centrifugal pumps. A satisfactory 10,000 r.p.m. aniline pump delivering 20 gallons per minute at 900 p.s.i. was developed in 1943. The construction of a nitric acid pump proved to be much more difficult because of the special materials this oxidizer requires. Drives such as electric motors and gas turbines were also investigated.[2, 38]

The Project undertook to test for Aerojet, during the Summer of 1943, two unusual proposals for supplying propellant to a long-duration rocket motor (30 minutes at idling thrust, 5 minutes at full thrust). The first would obtain pump drive from rotating rocket motors mounted so that a component of thrust would be made available for delivering torque - the system was called a Rotojet. The second used the principle of the Rotojet with a built-in centrifugal pump. A single combustion chamber was equipped with multiple angled exhaust nozzles. Cooling passages around the chamber walls served as pumping ducts when the whole assembly rotated--the system was called a Centrojet. Summerfield, at that time on leave from the Project, supervised the program at Aerojet. He believed that these systems were mainly the result of the interaction of the minds of William Van Dorn and Waldemar Mayer. It was decided to try them after a committee that included von Karman and Tsien asserted that they were the lightest and most efficient approaches, after reviewing a series of analytical studies of competitive schemes. (These included: gasoline-engine driven pumps, a gas turbine drive, a propellant steam jet pump, a direct combustion gas pressurization scheme, etc. The surviving scheme today is the propellant gas turbine drive. It was used successfully first by the German V-2 group in

1938 but this was not known to us.) Models of the Rotojet and Centrojet were constructed, but tests showed that, although the two systems worked in principle, the mechanical difficulties encountered were so great that there was little hope for them in practice.[2,40,41]

VI. FLIGHT TESTS OF ROCKET ENGINES

I pointed out earlier that the program of the Project was to develop solid and/or liquid propellant rocket engines for application to the "superperformance" of landplanes, including rocket assisted take-off, and abnormally large accelerations and increased flight velocities or rates of climb for only short periods of time. The program was launched on the basis of preliminary studies of the validity of using rocket engines for these purposes.[11, 12] C. B. Millikan and H. J. Stewart in January 1941 made a detailed analysis of the effect of auxiliary rocket propulsion on landplane performance.[42] A supplementary analysis was made by C. F. Fischer, a Navy officer, for his GALCIT master's thesis.[43] The predictions made by these analyses awaited experimental verification.

Ercoupe Flight Tests With Solid Propellant JATO Units

A message was sent to the Air Corps in the spring of 1941 that we were ready for flight tests of an aircraft equipped with solid propellant JATO's each delivering around 28 lb. thrust for about 12 seconds (cf. Section III). The Air Material Command selected the Ercoupe low-wing monoplane, bearing the designation YO-55, for the tests and selected Homer A. Boushey, Jr. (then a Captain) as the test pilot. Boushey in 1941 was doing graduate work at GALCIT and also acted as liaison officer between the AAF and the Project. An analysis of the performance and flight characteristics of the Ercoupe and of the manner of installing multiple JATO units designed by the Project was made by C. F. Damberg and P. H. Dane of the Army Air Corps, as their GALCIT master's thesis.[44]

The Ercoupe was flown from Dayton, Ohio to March Field, California, at the end of July 1941, where modifications were made for installing the JATOs. The flight test group consisted of the following: (Project personnel) J. W. Parsons, E. S. Forman, F. S. Miller and myself, as director of the tests; (AAF personnel) Capt. H. A. Boushey, Jr., Cpl. R. Hamilton and Pvt. Kobe (Figure 15). Von Kármán and C. B. Millikan joined the group at various stages of the program and some of the tests were witnessed by W. F. Durant, Chairman of N.A.C.A. Jet Propulsion Committee, and Fischer of the Bureau of Aeronautics of the Navy Department.[30]

A front view of the Ercoupe is shown in Figure 16 with three JATOs installed under the wing on each side of the fuselage (cf. Figure 11). Each JATO was mounted on

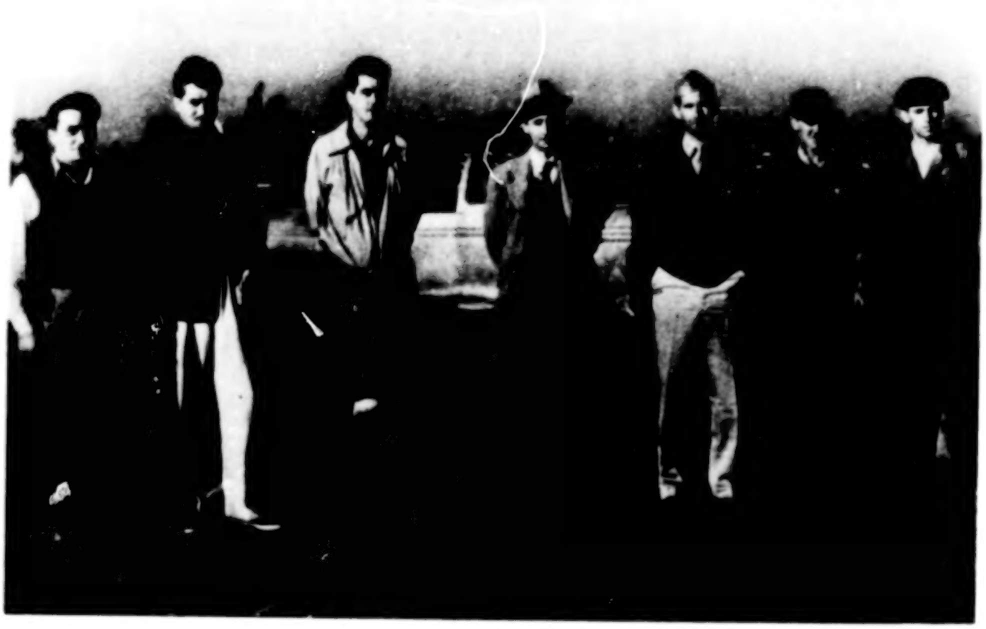

Fig. 15
Ercoupe Flight Test Group: (Project Personnel,
Left to Right) F.S. Miller, J.W. Parsons,
E.S. Forman and F.J. Malina,
(AAF Personnel, Left to Right)
Capt. H.A. Boushey, Jr.,
Cpl. R. Hamilton and
Pvt. Kobe

slides and retained by cotter pins that sheared if the JATO failed and blew off the exhaust nozzle, in order not to transmit shock to the airplane wing. That is, the reaction of the nozzle moving backward would project the JATO combustion chamber forward. The test program began very cautiously, since we had to anticipate possible explosive failures of the JATOs, and it was not certain that the Ercoupe would behave satisfactorily when rocket thrust was applied. Boushey, for one of the preliminary tests, flew the airplane equipped with one JATO on each side to an altitude of about 3000 ft. and ignited the rockets. We could not see the Ercoupe but our satisfaction in seeing a white jet trail appear in the sky was ended in a few seconds when a big bang reached us - one of the units had failed again. There were some anxious moments until we could see Boushey flying back and landing safely. Happily, the installation allowing for failure of the JATO without damage to the airplane had worked as planned.

On August 7, among the last ground tests conducted with six JATOs installed, one failed. Unfortunately, the exhaust nozzle rebounded from the runway and struck the

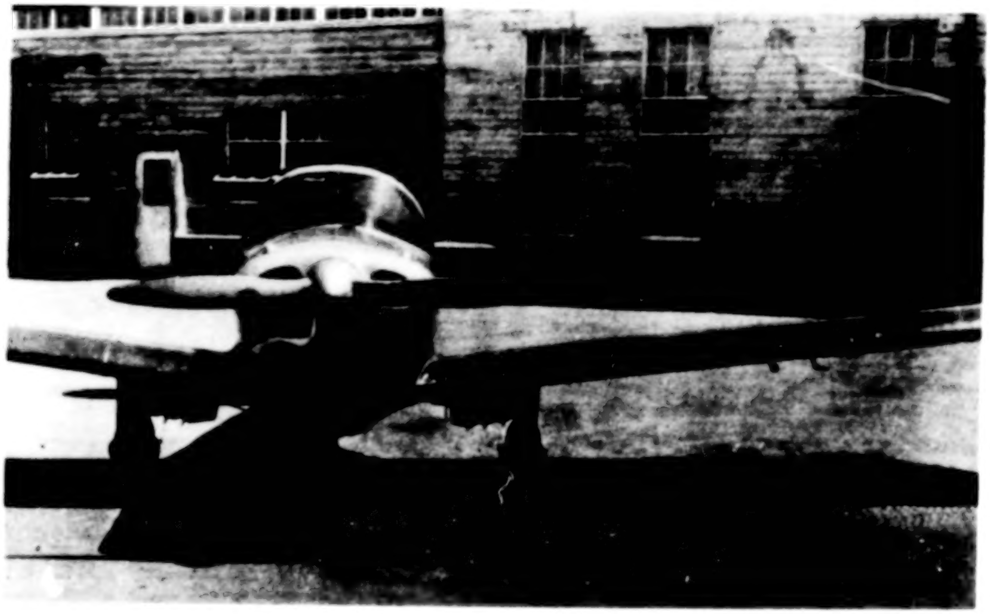

Fig. 16
Front View of Ercoupe With Three JATO's Attached
Under Each Wing

fuselage, ripping a 1-in. hole in the skin and scorching a longeron. The combustion chamber flew about 100 ft. ahead of the airplane before hitting the ground. Some damage was also done to the attachment installation. The report on the flight tests[37] contains the following laconic comment: "The pilot deserves credit for his willingness to continue flight tests as soon as the airplane was repaired." Much to everyone's relief there were no further explosive failures of the JATOs for the remainder of the test program during which 130 units were fired.

On August 12, 1941, Boushey made the first take-off of the Ercoupe with six JATOs firing. A view of a take-off is shown in Figure 17. The salient results of the test program are summarized in Table I. They were found to be in reasonable agreement with the theoretical predictions made by Millikan and Stewart. It will be noted that the use of JATOs reduced the take-off of the Ercoupe to about one-half of the distance normally required. The flight characteristics of the airplane were not significantly affected.

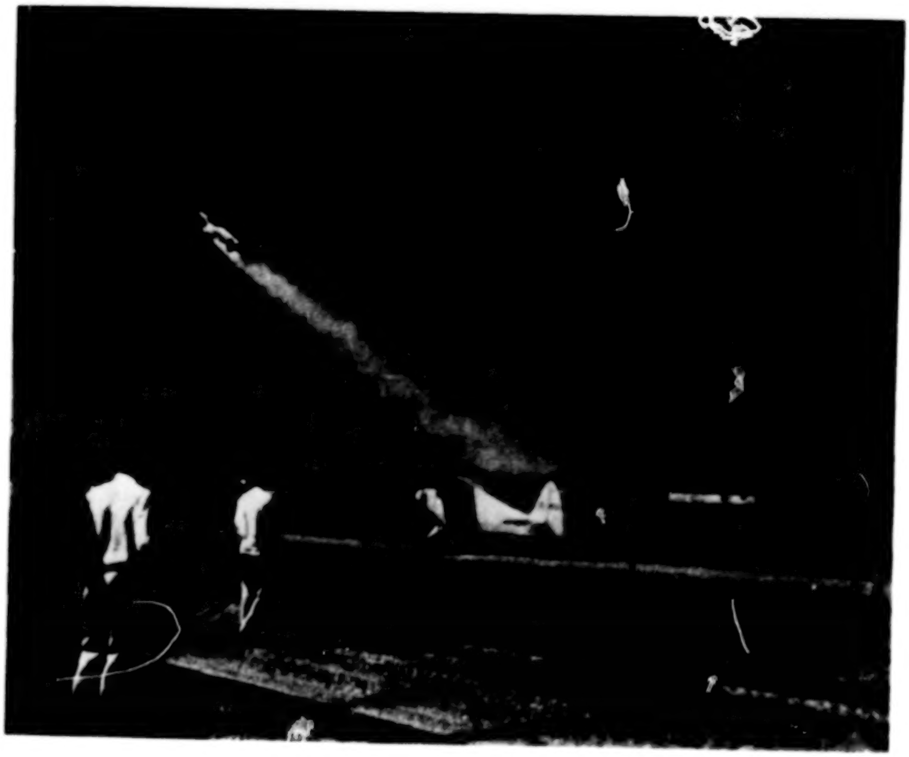

Fig. 17
View of Ercoupe Take-Off Assisted
By Six JATO Units

TABLE I

Item	Ercoupe without JATOs	JATO thrust lb.	New value of item	Percent saving
Take-off distance, ft.	580	170	300	48.3
Take-off time, sec.	13.1	170	7.5	42.8
Distance to clear 50 ft. obstacle, ft.	950	170	550	42.1
Distance to take-off with 285 lb. overload, ft.	905	166	438	51.6
Time to take-off with 285 lb. overload, sec.	18.8	166	9.5	49.4
Maximum indicated air speed at 11,400 ft. m.p.h.	62	171	97	56.5 (increase)

The first American manned flight of an aircraft propelled by rocket thrust alone was made by Boushey on August 23, 1941 (Figure 18). The propeller of the Ercoupe was removed and 12 JATO units installed, however, only 11 functioned. The Ercoupe was pulled by a truck to a speed of about 25 m.p.h. before the JATOs were ignited. The airplane left the ground and reached an altitude of about 20 ft.[30] This flight was not originally scheduled but we could not resist the opportunity to make the improvised demonstration of a future possibility of rocket propulsion.

Fig. 18
View of Ercoupe About To Take-Off With
Rocket Propulsion Alone

A-20A Flight Tests With Liquid Propellant JATO Units

We anticipated the flight tests of the RFNA-aniline rocket JATAs on the A-20A airplane with a degree of confidence, for we had gained much experience during the flight tests of the solid propellant JATOs on the Ercoupe. Nevertheless, with the lives of the pilot and the JATO operator in our hands, we proceeded with caution. The A-20A, after certain structural reinforcements had been made by the AAF Aircraft Laboratory at Wright Field, was flown by Major P. H. Dane to Lockheed Airport, Burbank, California, in March 1942. Here the JATOs were installed, whereupon the airplane was flown to the U.S. Air Force Bombing and Gunnery Range, Muroc, California.[27] The only difficulty that we encountered occurred during the first static tests; the unit in the starboard-side nacelle failed to deliver rated thrust. After several days of bafflement we found that the fault lay in the check valves of the propellant lines, which were thereupon eliminated in both units as unessential.[18]

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The principal members of the flight test groups were the following: (Project personnel) M. Summerfield, W. B. Powell, E. G. Crofut, B. M. Forman, R. Terbeck and the author, director of the tests; (AAF personnel) Major P. H. Dane, M. G. Cassell and L. A. Brady and (Civil Aeronautics Administration) E. N. Fales, J. Matulaitis and N. N. Rubin. Key personnel in charge of the flight tests are shown in Figures 19 and 20, and the pilot and JATO operator in Figure 21. Von Kármán and C. B. Millikan joined the Project group during parts of the program and Fischer observed some of the flight tests for the Navy Bureau of Aeronautics.

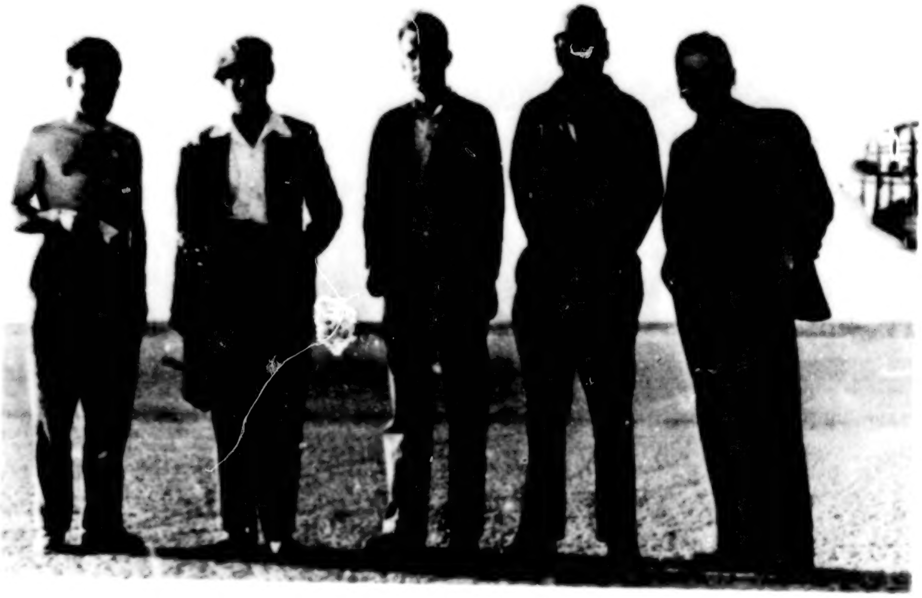

Fig. 19
Personnel in Charge of A-20A Flight Tests
(Left to Right) M. Summerfield,
F.J. Malina, W.B. Powell,
Major Paul H. Dane and
Th. von Kármán

The take-off tests were made from the bed of Muroc Dry Lake, on which a stripe 3 ft. wide and 10,000 ft. long had been laid out to guide the pilot. Fischer flew in from his Navy base at San Diego, California, in the midst of the take-off tests one morning when von Kármán joined us. He asked von Kármán if he would like to see the equipment in the cockpit of a modern Navy fighter. Those of us who knew von Kármán's reputation for leaving experimental apparatus in a mess after visiting a laboratory wondered aloud if Fischer's invitation was a wise one. Von Kármán climbed into the cockpit, and Fischer explained the purposes of the various instruments while I stood on the lower wing and

Fig. 21
Major P.H. Dane, Pilot, and B.M. Forman, JATO
Operator, During A-20A Flight Tests

Two tests were also made to measure the increase of indicated air speed in level flight with the JATOs ignited. At 5,000 ft., the speed increased from 252 to 300 m.p.h. or 19%, and at 10,000 ft. from [illegible] to [illegible] m.p.h. or 17.[illegible]%. The gross weight of the airplane was about 18,000 lb.

TABLE II

Item (A-20A at 20,000 lb. gross weight)	A-20A without JATOs	JATO thrust lb.	New value of item	Percent saving
Take-off distance, ft.	[illegible]	[illegible]	1,670	32.3
Take-off time, sec.	[illegible]	[illegible]	[illegible]	[illegible]
Airborne distance to clear 50 ft. obstacle, ft.	[illegible]	[illegible]	1,1[illegible]	33.3
Airborne time to clear 50 ft. obstacle, sec.	[illegible]	[illegible]	[illegible]	27.8

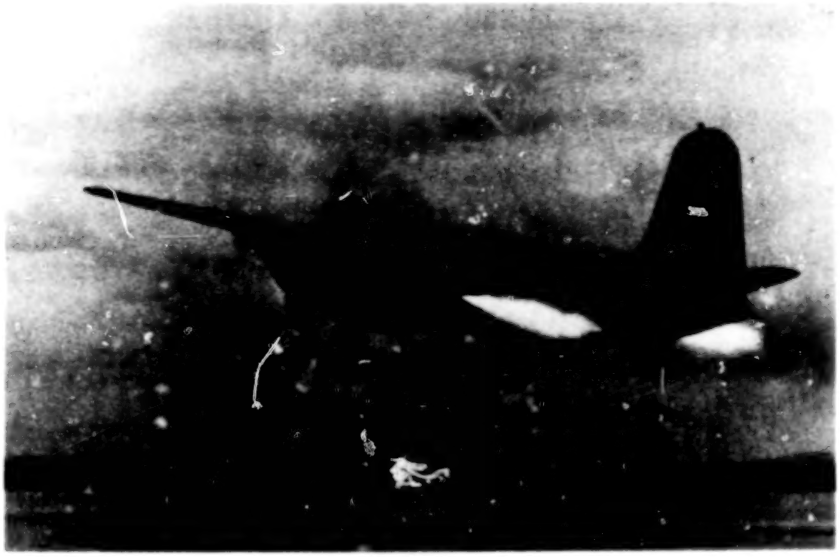

Fig. 22
View of A-20A Take-Off Assisted By 2 JATO Units

VII. ROCKET PROPULSION UNDER WATER

Qualitative experiments of rocket motors fired under water, made in the autumn of 1942, were reported upon by R. B. Canright. It was found that a RFNA-aniline motor started satisfactorily when submerged under 9 in. of water, even when water filled the motor and part of the propellant lines. Solid propellant units were tested when submerged at depths from 2 to 6 ft. of water in the lake formed at Morris Dam, California. The results showed that these engines could be operated under water as JATOs for flying boats, and for the propulsion of torpedoes. A patent for the application of rocket propulsion to water-borne vehicles was granted to Summerfield and the author.[46] The Armament Laboratory of the AAF Air Technical Service Command at Wright Field, upon hearing of these tests, requested the project to submit a proposal for development of a "hydrobomb." It was to be an air-launched torpedo, but, in order to circumvent the monopoly held by the Navy for the development of torpedoes, the AAF decided to call it by a different name. Propulsion of the missile was to start after it entered the water.

Von Kármán and I, in a memorandum dated February 20, 1943, proposed the design, construction and operation of a towing channel for underwater rocket propulsion research.[47] The proposal also included research on the design of a hydrobomb. When we explained the proposal to General Chidlow in Washington, D.C. his comment was: "The next time you come to see me you will want money to put rockets on my swivel chair." The proposal was accepted by the Armament Laboratory, and the work carried their designation: Project MX 363. Our designation for the work was GALCIT Project No. 2. An underwater Propulsion Section was established and placed in the charge of Dunn, who in 1944 became Assistant Director of JPL. Under Dunn's supervision, the towing channel or hydrodynamic tank was built. It had a length of 500 ft., a width of 12 ft., and a depth of 16 ft. (Figure 23).[3] The towing carriage was, at my suggestion, to be driven by a controllable RFNA-aniline engine delivering a maximum thrust of around 3,000 lb. to give a carriage speed of around 40 m.p.h.[48]

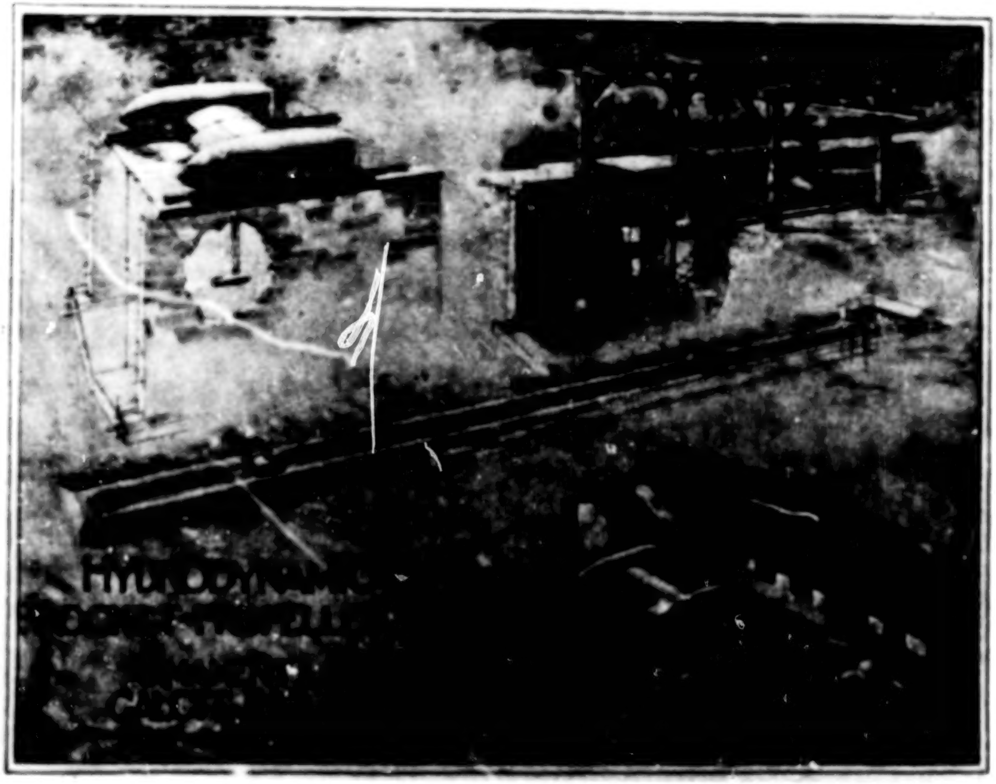

Fig. 23
Drawing of Hydrodynamic Tank Installation

The engine for this first rocket-propelled car in the U.S.A. was designed by Powell and Crofut.[49] The engine had three 1,000 lb. motors and a gas pressure propellant supply system. One memorable day in 1943 I was invited to watch a static test of the engine mounted in the completed towing carriage (Figure 24). George Emmerson, our brave photographer, and I posted ourselves to the rear and to one side of the carriage. We heard the order to fire the engine and to our horror, almost immediately saw one of the

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

effect of the rocket jet upon cavitation. A special solid propellant and motor were designed by the Project for the full-scale hydrobomb that would withstand water impact when the missile was launched up to speeds of 400 m.p.h. Launching tests were made at the Torpedo Launching Range developed by Caltech for the Navy at Morris Dam, California.[50, 51]

VIII. FORMATION OF THE AEROJET ENGINEERING CORPORATION

It became evident in 1941, following the successful flight tests of the Ercoupe and with good progress being made in the development of a liquid-propellant JATO, that steps would soon have to be taken for the production of JATOs for the Air Force and the Navy. Caltech, being an institution for education and basic research, did not appear to us to be appropriate for undertaking engineering development and production on a large scale. Furthermore, I shared the opinion of Parsons and Forman that after the efforts we had made during the previous five years we should participate in the exploitation of our ideas. I proposed to von Kármán in September 1941 that we try to initiate the production phase of rocket engines, and found him sympathetic. He pointed out that since he and I were members of the faculty of Caltach, there probably would be objections made to our becoming businessmen and there certainly were some. Robert A. Millikan, with his usual broad outlook, expressed concern as to whether we could manage a commercial organization successfully.

To minimize these objections, the first plan was to try to get an existing aircraft company to set up a rocket engine division, with a special arrangement for our participation in its work and in the sharing of profits. Von Kármán describes in some detail in his autobiography[9] these unsuccessful efforts. Leaders of the aircraft industry in Southern California foresaw no future for rocket propulsion! Then, upon the counsel of Andrew G. Haley, von Kármán's attorney, we decided to found a company of our own, after a favorable discussion of the idea with General Frank C. Carroll at Wright Field.[52] The Aerojet Engineering Corporation, now called the Aerojet-General Corporation, was organized at the end of 1941 and formally incorporated on March 19, 1942, with the following officers: Von Kármán, President and Director; Malina, Treasurer and Director; Haley, Secretary and Director; Parsons, Forman and Summerfield, Vice-Presidents. Our first capital contribution to the company amounted to $200 each. Those of us who held patents assigned them to the company.

It was no easy matter to decide who of those connected with the Project should be invited to join us in the venture. After the company was underway, C. B. Millikan especially felt left out. A year later we decided to offer him some shares for purchase, which he bought, and then he actively aided with the development of the company. Parsons, Summerfield and Forman, by the end of 1942, spent much of their time at Aerojet, assisting with the transition from the experimental stage to pilot scale and to full-scale production of solid and liquid-propellant JATO units. In September, Haley took over as president of the company and von Kármán and I again concentrated our efforts on the continually expanding program at the JPL Air Corps Project.

This changeover was prompted, in part, by the attitude of the AAF to our becoming businessmen. Von Kármán has the following story in his autobiography.[9] We received word from Wright Field that the Air Force had decided not to renew the first contract of Aerojet for liquid-propellant JATOs. Somewhat annoyed, he and I flew to Washington, D.C., to find out what was wrong. Our old friend, General Ben Chidlaw, told him in no uncertain terms the following: "We like you very much, Doctor, but only in cap and gown to advise us what to do in science. The derby hat of the businessman does not befit you."

The problem of "hats" haunted us during the next years. At this time, von Kármán and I were actually alternating three hats - we were on the staff at Caltech, at the governmentally-owned JPL operated by Caltech, as well as officers of Aerojet. And von Kármán had several other hats; for example, he was retained as a consultant by the Northrop Aircraft Company. The more strenuous objections to possibilities of our having conflicting interests, however, were soft-pedalled because there were so few qualified persons in the country to deal with the required expansion of rocket propulsion development and production. Close technical liaison was maintained between the Project and Aerojet until 1944 when the General Tire and Rubber Company bought a majority interest in Aerojet from the founder shareholders. This sale was forced upon us because, as the government told us, we had by then the lowest ratio of invested capital to contracts of any company in the country. Thereafter, the company concentrated more and more on production rather than development and its relations with JPL became more and more tenuous.

IX. JET PROPULSION ENGINEERING EDUCATION

In 1943 von Kármán organized at Caltech for the AAF Material Command, the first graduate course in jet propulsion engineering in the U.S.A., utilizing the staffs at GALCIT and JPL. The course, at first, was limited to officers of the Army and Navy, but later opened to selected civilian students. Lectures in the course were collected in 1946 by the Air Technical Service Command under the title "Jet Propulsion".[2] The 799-page volume was edited by Tsien and contained contributions from: P. Chambre, J. V. Charyk, L. G. Dunn, A. Hollander, N. Kaplan, Th. von Kármán, F. J. Malina, C. B. Millikan, M. M. Mills, A. J. Phelan, W. D. Rannie, H. S. Seifert, H. J. Stewart, R. F. Tangren and H. S. Tsien.

This volume exhibits, especially, the great progress made between 1939 and 1946 in the U.S.A. in the development of jet propulsion engines of various types on a firm scientific basis. The popular conception has been built up that developments during this period in Nazi Germany far outdistanced American results of research on the fundamentals of rocket propulsion. That conception is false, for when we studied German developments after the war we found that, as far as liquid propellant rocket engines were concerned, they had more experience only with the practical aspects of large-thrust LOX engines. Developments in the U.S.A. of composite long-duration solid-propellant engines were much more advanced. The tendency in the U.S.A. to worship prophets from afar was well demonstrated in the case of rocket propulsion developments during this period, with some unhappy historical effects.

The approaching end of World War II posed serious policy decisions for Caltech as regards JPL. Some of the problems were brought to the fore by von Kármán in a memorandum he prepared in 1944 for the consideration of the Caltech Trustees on the possibilities of the establishment of a Jet Propulsion Laboratory owned by Caltech.[53] Von Kármán succeeded in convincing Caltech to accept the ORDCIT contract with the Ordnance Department, with concurrence of the AAF, to initiate the first research program on long-range rocket missiles in the U.S.A.[54] He was, however, also concerned with the post-war future of jet propulsion research on a permanent basis. He wrote: "It has now become known that one of the great changes in aviation equipment introduced by wartime research will consist in the use of jet propulsion as motive power. The present jet propulsion equipment is yet of a rather crude nature. However, certain very definite results have been obtained and promise wide possibilityes of application both in militery and civilian aviation."

Von Kármán, Summerfield, Tsien, and the author performed a comparative study of jet propulsion systems as applied to missiles and transonic aircraft during the winter of 1944. We compared solid and liquid propellant rocket engines, and thermal jet engines such as the aeropulse, ramjet, and turbojet for various applications.[55] Under the contracts with the AAF and the Ordnance Department, the Laboratory soon involved itself with research on the full spectrum of jet propulsion engines. What is more, JPL became the largest single operation to be administrated by Caltech. The work I initiated with the GALCIT Rocket Research Project in 1936 had blossomed within eight years into a major activity of the Institute. The many problems arising from a private educational and research institution administrating large scale research for the government and for military applications after the war, ranging from using the staff of Caltech on such research, to allowing its staff members to become involved in industry, and to introducing within the curriculum the subject of jet propulsion engines, were all upon us.

Von Kármán took a leave of absence from Caltech at the end of 1944 to establish the AAF Scientific Advisory Group in Washington, D.C.[9] During 1945, I devoted more and more time to the problems of the Caltech-JPL relationship. I submitted, in November 1945, a memorandum on the future of jet propulsion research at Caltech.[56] My primary objective was to assure the survival of JPL within the structure of the Institute. To this end, I proposed that the Institute establish a Department of Jet Propulsion Engineering, with its own funds, and that JPL be operated as a government facility under the new department. The starting point of the department would have been the graduate Jet Propulsion Course begun in 1943. Although the proposals I submitted were not adopted in the way I envisaged, Jet propulsion engineering education and research were added to Caltech's program, and JPL continued with a somewhat more tenuous link to the Institute.

At a meeting of the Society for the Promotion of Engineering Education at Berkeley, California on February 22, 1946,[57] I discussed the affect upon engineering education of jet propulsion and the beginnings of astronautics, and concluded with the following statement: "It appears that jet propulsion developments have served, in some measure, to increase the present pressure for a careful evaluation of the curricula and general spirit of engineering education. The need for men trained as research engineers to aid in bridging the gap between scientific research and useful application of new knowledge of nature has been critically appreciated by those given the responsibility for carrying out difficult phases of an urgently needed development during the war."

X. CONCLUDING REMARKS

I look forward to the opportunity of presenting at a future symposium of the International Academy of Astronautics my third and last Jet Propulsion Laboratory memoir. It will deal with the ORDCIT Project, from its inception in 1944 to the end of 1946. It was the first long-range rocket missile research undertaken in the U.S.A. and it also permitted resumption of rocket research for space exploration initiated at Caltech in 1936.

The help I received from Martin Summerfield and Walter B. Powell on technical parts of this memoir, and from George Emmerson in collecting photographs, is highly appreciated.

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DEVELOPMENT OF THE GERMAN A-4 GUIDANCE AND CONTROL SYSTEM
1939-1945: A MEMOIR[+]

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INTRODUCTION

The guidance of launch vehicles and spacecraft can be subdivided into two partial objectives: a) stabilize the spacecraft in the direction of its flight, and b) carry out the navigation or flight control function enabling spacecraft to reach their destination. These also make possible the navigation, guidance and flight control of conventional airliners, so it is no wonder that in 1939 the first rocket guidance systems corresponded closely to the techniques of airplane flight control modes and the inertial stabilization systems of ships. During the flight of missiles, however, flight regions are reached where the air density becomes too low to provide strong enough aerodynamic forces and moments for stabilization and flight path damping. Therefore, artificial damping modes have to be introduced to compensate for the imperfections of the guidance components, of radio signal disturbances and related complications, as well as the flight path variations they cause. This necessity led to the introduction of reaction control systems and electronic guidance components which could better provide the needed additional corrections.

With the increase in flight altitude above the Earth's surface, and of the presence of gravimetric anomalies in the gravitational field of the Earth, however, these measurements are insufficient. They require the introduction of a reference system independant of such anomalies to secure the exact definition of the flight path, and the instantaneous, true position of a launch vehicle or spacecraft, respectively. Thus, one must either introduce an inertial measuring system or apply precise and reliable ground tracking of the spacecraft or launch vehicle position, or combine both of these methods.

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The first candidate for the solution of this problem involved further development of gyro stabilization techniques to obtain a three-axial reference system that is space-fixed and not dependant on continuous tracking of the projectile or spacecraft position, or, in other words, is independant of the gravity-field variations. Gyros, by themselves, cannot accomplish this. One has also to be able to follow the state of the motion of the reference system in the spacecraft in relation to its three inertial motion axes and to its three rotational axes, in order to be able to describe precisely the trajectory or flight path of the launch vehicle or spacecraft. This can be accomplished either with body-fixed gyros installed in the projectile to measure rotations and rotational rates of the reference axes of the vehicle—as this is done conventionally in airplanes—or with equipment to monitor the motions in the directions of the body-fixed axes with integrating accelerometers that by two fold integration of the acceleration, describe the respective distances travelled in the directions of the axes inertial axes. Because, at the same time with these motions, rotations can take place, and the determination of the actual trajectory becomes very complicated and requires extremely precise calculations. Only during the past decade has this been satisfactorily achieved by on-board digital computers, without adding too much to the weight of ballistic and spacecraft vehicles. However, where the control system prevents the rotation of two of the axes, and the rotation of the third axis amounts to less than 180°, the task of the control system to keep the deviation for the two lateral axes to a minimum, for ballistic missiles particularly, a substantial simplification can be achieved.

This last described technique can be applied to preprogrammed ballistic missile and launch vehicle trajectories, and can perform the corrections for indicated deviations from the standard flight path after the separation from the launch vehicle using the spacecraft inertial guidance system. Russian launch vehicles, as well as the Saturn system, apply this solution. The Saturn system has a triple gimbaled inertial platform, on which, besides stabilizing gyros, three velocimeters with integrators also operate. In the case of the Saturn system, the gimbal-positions are measured with digital pickups and reported to the digital launch vehicle control computer. This system makes position and trajectory guidance available from the same reference system, But for the present, let us return to the history of these systems.

The necessity to create a new reference system fixed in space and independent of Earth reference points caused Professor Schuler, in Goettingen, for the first time to propose a stabilized ship gyro platform, to guide ships for long periods, independent of the motion of the sea and also during darkness and absence of visibility of the stars, on a constant, precalculated course. Such ship gyros were developed according to Schuler's proposals, under the direction of Captain Boykow, by the Krieselgeraete A.G. in Berlin, and introduced into maritime navigation. Since the gyro retains its rotational plane precisely, the larger the ratio of the gyro impulse to the disturbing impulse the better, and so very heavy gyros with high impulses were used for the stabilization of the ship gyro compass equipment.

In the 1920s Professor Schuler also developed a theory explaining that a gyro pendulum is independent of accelerations affecting its suspension point when the imaginary length of the pendulum corresponds to the Earth radius. Today such a pendulum is called a "Schuler tuned pendulum." The period of such a pendulum is 84 minutes, and for many years the Schuler pendulum technique has found application in the inertial navigation of airplanes. However, to arrive at the expected guidance precision with weights acceptable for flight instruments, the original ship gyro units had to be made much smaller, and their drift rates also had to be reduced significantly. This effort took many years and led to the replacement of the ball bearings for the gimbal supports of the gyros in their sensitivity axis by liquid suspension or gas bearings, with very refined premium ball bearings used in the less critical applications. It was Boykow who suggested the use of accelerometers for air navigation. Tests with such gyro stabilizers combined with accelerometers began in 1930, and were flight tested later.

But large navigation deviations were observed during these tests because twofold integration of even very small residuals in the measurement of the acceleration-vector led very rapidly to large error readings of the integrals, and the gyros available between 1930 and 1940 were by no means ready for this measuring objective. However, investigators realized that as soon as the quality of the gyro stabilization and accelerometers improved, this method would become very promising. This disadvantage, however, appeared much less severe for applications in launch vehicles and ballistic missiles because they possessed much shorter propulsion and flight durations. It appeared to be the way to become completely or partly independent of radio tracking and electronic navigation aids, since these often showed anomalous operating conditions and could also be influenced by electronic noise or even countermeasures.

In 1939 aircraft navigation employed compass-supervised directional and azimuth gyros (or artificial gyro horizons)—"supervised" by an erection pendulum. They made instrument flying possible, but were too inaccurate for navigating long flights unless the actual flight path could be "updated" through radio bearings. Continuous monitoring of the gimbal angles of the horizon platform by the Earth's gravity vector makes the gyro platform independent of the Earth's rotation since the vertical reference vector is always again restored by the erection pendula. This technique, however, is also not applicable in satellites or spacecraft moving under physically weightless conditions, since the gravitational force is just compensated by the centrifugal force, voiding the supervisory effect. On the other hand, such a platform will retain an inertial orientation or "space-fixed axis," if drift rates can be compensated or remain within adequate limits.

THE CONTRIBUTION OF PEENEMUENDE

Requirements for the A-4 Flight Control System

When the first ballistic missiles were developed at Peenemuende in Pommerania, inertial guidance systems were not far enough advanced to take over the guidance task of the A-4 missiles. However, the powered trajectories of these missiles lasted only sixty-seven seconds, as contrasted with launch vehicles designed to deliver satellites or spacecraft, and so made the use of inertial guidance components less problematic. The powered portions of the flight were subject to high accelerations, compounding the problem of systems errors, but the short time of powered flight in terms of error build-up for first and second integrals of the acceleration made these effects less severe. Therefore, it appeared possible to use accelerometers in an inertial reference system, permitting ballistic missiles to become independant of fixed radio guidance stations.

The A-4 guidance system established the burn-out location of the A-4 powerplant at about 38-km altitude. The altitude tolerance as well as the velocity vector tolerance had to be small enough to limit the dispersion of the impact location. We sought to fly the missile along a precomputed optimum trajectory without guidance and control errors, allowing impact at the desired range and direction (ballistic trajectory). The accuracies of the components of the guidance equipment and the thrust dispersions of the powerplant, including weather anomalies, led to a three Sigma dispersion of less than one tenth of a percent of the range for the various guidance and propulsion cut-off techniques under consideration. In practice errors do exist, and the propulsion schedule is not perfectly maintained. The accuracy of the powerplant cut-off velocity depends on the absolute thrust level of the rocket engine, of the closing velocity of the fuel inlet valves, of the hydraulic resistance of the fuel lines and injection nozzles between the valves and the combustion chamber, and also of the hydraulic resistance of the cooling jacket of the rocket engine, to name a few of the more important contributors to thrust dispersions. These factors, besides nozzles exit resistance, mainly influence the execution time-delay of the cut-off signal from the burnout velocity control subsystem of the guidance and navigation system to the powerplant of the missile, and make thrust decay a function of time. The latter therefore is subject to dispersions. To this dispersion one must add the value of the uncertainty of velocity and altitude determination of the cut-off point, caused by cumulative errors of the control, guidance, and navigation system itself.

To assess the magnitude of the initial errors, one has to establish an error budget within which the various potential error source ranges have to be listed and confirmed by tests. The dispersion tolerances can be established by mechanical improvements and selective groupings of interacting subsystems. In the case of the A-4 the development of new fuel valves of extremely short reaction times achieved the desired minimum dispersion of the burnout velocity. We effectively subdivided the velocity cut-off process into two stages: with a pre-cutoff signal, the thrust was reduced from 30 to 8 tons, resulting in a small residual longitudinal acceleration, causing the missile to "creep" towards the normal cut-off velocity. This signal was followed by the main cut-off signal which entirely terminated the thrust. The acceleration then went through zero, and the altitude and direction of the velocity vector assumed a mean tail-off value. All the cut-off errors remaining at this point would retain the impact point including wind dispersions during the reentry of the projectile within the acceptable dispersion range (- .1% of range for longitudinal and lateral displacement). This requirement still exists today for the delivery of spacecraft or payloads into a chosen orbit, but the required tolerances needèd to meet the desired target accuracy of the ephemeris data would have to becomè still much smaller.

To solve the cut-off problem at Peenemuende we sought to improve the weight of maritime gyro platforms, and of artificial horizons and directional gyros of aircraft. The latter consisted of gyros of two degrees of freedom. There were no operational accelerometers possessing the required accuracies to meet the A-4 tolerance objectives. Considering the accelerations in powered flight, the gyro horizons produced the desired accuracies only if the erection pendula were activated, while the resulting gyro drift rates become intolerably high whenever the erection system was turned off. These methods therefore required modifications to eliminate the described disadvantages and permit the maintenance of the local vertical automatically in inertial space.

Other methods to accomplish the task involved radio tracking and radio guidance methods. They allowed flight velocity and direction to be continuously measured and updated. Theoretical investigations showed that the radius vector velocity parallel to the burnout point tangent on the flight path, measured by the doppler velocity between a transmitted signal and its return from the missile, would accomplish this. A signal transmitter on the ground, a highly stable transmitter of equal frequency on board, synchronized before take-off, achieved this. Use of this technique required a frequency bridge of a low time constant and high precision to obtain the necessary accuracy without intolerable signal infidelity. High measuring accuracy and extremely low time constants are normally diametrically opposing qualities. In the case of achievement of this rather difficult objective, the "beat-frequency" between the two signal sources would be the velocity vector from the ground transmitter to the flying missile. Further analysis showed that the two transmitter solution required a high degree of frequency stability between the two transmitters to make this method workable. However, a solution, multiplying the frequency received on the ground and retransmitting on the new frequency, while the ground-transmitter compared the signal returned with the ground multiplied frequency, achieved its objective. In this case, the otherwise unavoidable frequency drift between two transmitters was avoided and this method was adopted in practice.

This method did not permit the determination of the actual altitude or of the lateral displacement of the rocket vehicle. Additional range measuring equipment, placed in direction of the flight path plane and lateral to it, was needed to provide this data. Use of radio-navigation equipment would also make it possible to measure the deviation from the flight path plane, and, if combined with range measurement (as with a radar), provide also the approximate position and velocity in space. Additional gyros would therefore permit improvement of all measured data by the introduction of corrections.

Development of the A-4 Flight Control and Navigation System

In fall of 1939, we decided to begin development of several A-4 flight path determination and monitoring techniques based on the use of gyro and inertial references as well as of electronic ranging and tracking equipment, using concurrent single and multiple integration of flight path accelerations to compute instantaneous velocity and range vectors. We chose this approach because it was not obvious which of the considered methods or combinations of methods would be successful, particularly since the radio methods could be very much affected in their accuracy by intentional electronic countermeasures.

To plan this work on a broad basis, a number of outstanding German and Austrian university professors were invited to participate. They attended lectures concerning the flight and operational environment in which the "measuring equipment" would have to work, and observed fullscale powerplant tests on test stands. The professors unanimously concluded that the announced task to measure vehicle position and instantaneous velocity vectors accurate enough to achieve a dispersion not exceeding the value of one-tenth of one percent of the range was clearly outside the state of the art of that time, and was, therefore, impossible. However, we asked these professors to reexamine the problem.

Eventually five of the originally invited professors returned after three months and proposed solutions to the problem. These solutions were fundamental to this problem and still today belong to the methods recognized as superior for space guidance and navigation applications. This can be termed a surprising turn of events when compared with the outcome of the deliberations and conclusions of the first session. The progress made since 1939 is represented by the improvement of the achievable accuracy or reduction in dispersion for gyro subsystems and integrating accelerometers by a factor from 10^3 to 10^4, while radio guidance and inertial navigation methods proposed even today can be met or surpassed only by use of atomic frequency standards and with digital computers of high accuracy. Some of these very pessimistic professors, as time went on, became enthusiastic participants in the search for solutions to problems which the young space guidance and navigation discipline was facing in 1939. Their contributions will be discussed later in some further detail.

As indicated earlier, accelerometers of rather high measurement precision, as well as angular rate and angular acceleration indicators which permit integration of their outputs, have played an increasingly important role in space navigation tasks. These have contributed substantially to the subsequent increase in achievable accuracy and the simplification of the equipment used for these purposes. All integrations, even if their initial errors affecting the integration constants are very small, can grow to very large numbers over time. Thus, high measurement precision and component quality have to be a major design goal. In 1930 an attempt was made to navigate an aircraft equipped with a gyrostabilized platform and mechanically integrating accelerometers mounted on it. The flight, which departed from Berlin-Adlershof, was discontinued and the attempt terminated after three hours of flying time when the aircraft equipment indicated a position somewhere in Australia, while visual observations confirmed the aircraft position to be at the western border of Germany, near Holland. The reason for this very substantial discrepancy lay in the lack of precision equipment.

The lack of knowledge about desirable subsystem properties for the design of such navigation subsystems to achieve the desired accuracy prevented the establishment of adequate design criteria. Only after 30 years of superior and costly development efforts would the state of guidance technology advance far enough to provide commercial aircraft the ability to navigate without radio aids over thousands of kilometers and flight durations of seven to ten hours, with an average position uncertainty of only one to two kilometers. From this example one can judge the developmental tasks we faced in 1939, and why the professors hesitated to associate themselves with such an apparently hopeless endeavor that only could be attempted with youthful optimism.

TECHNICAL SOLUTIONS TO THE SPACE NAVIGATION PROBLEM AND THEIR MECHANIZATIONS

Near the Earth's surface, using the local vertical of the gravitational field, the knowledge of orientation of the horizon, and the geographic North direction and an altimeter, one can determine aircraft position accurately. This also can be accomplished with gyros providing an artificial horizon, an accurate altimeter, and a gyro compass monitored by a magnetic remote compass, or by using a gyro stabilized inertial platform and applying again gravity and the magnetic North tie-in to correct or compensate for gyro drift, and the altitude above sea level. These techniques provide aircraft location and its vectors of motion with, for most purposes, adequate accuracy.

For greater distances from the Earth's surface, without access to accurate surface fixes and horizon references, continuous accurate navigation of a spacecraft and determination of [illegible] state of motions is only possible with additional well defined and accessible references, such as accurate orientation of the Earth horizon, or knowledge of the physical center of the Earth, of directions to the center of the Sun, and/or the Moon or remote stars or planets besides using inertial reference equipment. Therefore, in the case of the A-4, the German Rocket Research Center developed in parallel more or less sophisticated inertial and body fixed gyro references using singly or multiple integrating linear and angular accelerometers, as well as more sophisticated stationary and mobile radio navigation, radio beam- or plane-rider navigation systems, to monitor the flight control system of this ballistic missile. This was done to obtain two to three independent methods, using different physical concepts and a range of fabrication precision and costs, that could be optimized for a variety of navigation objectives.

The first group of this equipment combined an aircraft autopilot with fixed and mobile radio ranging and range-rate equipment. It used the radio position and velocity fixes to correct flight path deviations and permitted, with the aid of an inertial angular rate measurement, successful flight path stabilization. Another method, independent of radio navigation aids, consisted of strapped-down, airframe fixed, directional and rate integrating gyros, combined with accelerometers, integrating fully or partially the readings in all six degrees of motion states, and thus stabilized the flight path and rotational axes of the ballistic missile. By computing the burnout velocity of the rocket engine, referenced to a standard trajectory that combined target parameter tolerances which statistically averaged to errors of less than .1% of the range, we achieved this objective. Use of a standard trajectory permitted correction of deviations based on standard values only, and can be optimized so that the effect of the deviations is minimized.

The solutions to the standard trajectory deviations were not abstract (mathematically rigid), and we expected that the burnout errors, and consequently the target dispersion errors, would be large. Against all expectations, however, these simplifications of flight control and navigation equations permitted a closer and more accurate flight path control than would have been expected of these simplified control and navigation methods. The magnitude of the deviations was clearly a function of the thrust deviation from the standard engine thrust versus altitude relation, caused mainly by the degree of match or mis-match of the powerplant subsystems whose values are dependent upon calibration and quality-control, and subject of component matching and consideration of cumulative tolerances. Careful matching and selection of components influenced such deviations favorably.

The third kind of navigation and flight control system used a highly precise (for 1943) three-axis stabilized platform with two longitudinal accelerometers and one lateral accelerometer. The readouts were integrated once, and of one twice, to yield a powerplant cut-off velocity that approximately compensated for thrust deviations and burnout altitude errors or burning time deviations, respectively. In a sense, since the two longitudinal accelerometers integrated along two different axes slanted in the vertical trajectory plane, these vertical and horizontal velocity components actually had their greatest sensitivities based on a desired burnout trajectory tangent.

The flight path of an A-4 equipped with this fully inertial navigation system thus used a standard trajectory. The pitch deflection followed a highly precise precomputed time sequence which, for the nominal range, reached a burnout deflection of 42°, a value somewhat range dependent. To achieve this deflection, a precision timer rotated the base of the pitch-angle pickup so that its zero direction at all times pointed in the first approximation in the direction of the desired flight path tangent, reduced by the instantaneous angle of attack in pitch. To determine the proper time of burnout, one of the accelerometers was mounted space-fixed on the platform. Its measuring axis at takeoff was parallel to the precomputed burnout tangent, and remained so throughout the powered flight. The indicated first integral of its acceleration resulted in a velocity—which was characteristic of the burnout velocity even if the burnout altitude was not correct and would lead to a shorter or longer range—depending on whether the burnout altitude deviation was negative or positive.

The second accelerometer was inclined forward so that its axis at the launch table pointed to the nominal burnout point in space. Its time-related output was integrated twice and represented a distance characteristic for the location of the burnout point in x- and z- directions. If this point in time was reached later than at the nominal burnout time and the velocity indication was approximately correct, the missile flew a steeper trajectory and, if uncorrected, overshot its target. The burnout velocity of the first accelerometer therefore had to be reduced by an approximate amount to yield the proper range. Therefore a combination of the two accelerometer read-outs after their respective integration yielded the required corrections. In reality, this case was somewhat more complex than described since wind effects deviation of its inflight mass and variations of the drag of the missile, besides azimuth angle and thrust deviations, affect the result (effect of Earth rotation versus time of missile flight). The lateral accelerometer also compensated for lateral wind effects and dynamic oscillations of the missile due to gusts and lack of aerodynamic damping, by using the second integral of the lateral deviation to keep the lateral deviations as small as possible and, using the third integral, to compute a "crab angle" to compensate for wind effects, aerodynamic unsymmetries, etc. in the lower portion of the trajectory.

The sensitivity axes of the gyros normally were equipped with ball bearings elected for the lowest friction. To reduce the sensitivity axis friction even further, our two main contractors for these gyros chose fluid suspended bearings (Siemens) and gas bearings (Kreiselgeraete Gm. b. H.) to accomplish this objective, and applied it also for the integrating accelerometers. With the exception of a Northseeking Gyro, however, these developments did not reach serial production before the end of World War II (But the Northseeking Air Bearing Gyro was used during the war for artillery purposes.) Since the burning period for the A-4 missile amounted only to 67 seconds, the deviations of the integrating accelerometers were quite small despite the high "g"-levels of the trajectory (≤ 5 g), and almost achieved their design goals, but their reliability, including that of the control electronics, was not as satisfactory as we desired.

As an alternate to the third method, lateral deviation measurements from a radio guide plane were employed together with the gyro stabilized inertial platform system to reduce lateral dispersion. Besides the displacement itself, this arrangement also used the first and second derivative of the lateral displacement as well as the integral of the lateral displacement to compensate for wind effects and aerodynamic unsymmetries of the yaw plane. In addition, this system employed the all electronic analog flight computer (Mischgeraet or mixing computer), flight tested in aircraft before deployment in missile firings. We found that the natural frequency of the yaw response could be significantly increased, with a concurrent increase of the yaw damping, by giving lateral displacement, lateral displacement rate, and the rate of change of the lateral displacement into the roll channel of the flight control system. This latter technique was not applicable to the A-4, but would have been of great value for the winged A-8 vehicle. Since only two of these were flown without flight navigation, this technique was only applied to aircraft. The stability and effectiveness of this technique was amazing even at small distances from the guidance transmitter; the aircraft could fly stable directly over the transmitter at low altitudes without changing the gain-settings, optimum for the far field of the guidance plane.

Before lift-off of the A-4 missile, the gyro platform was erected by two pendula, a procedure which took between 20-to-30 minutes to satisfactorily level the platform. The erection system was switched off just prior to lift-off. At the same time, the displacement signal channels, zeroed out to prevent too early deflection of the control surfaces at lift-off, were switched on and the gain factors increased to their optimum value, which varied with the location on the flight path. All gyro, accelerometer, and integrator read-outs were approximately multiplied by the respective gain settings and mixed into the corresponding control axis channels by a fully electronic analog flight computer, and the signal outputs transmitted by wire to the servomotors at the jet vanes and aerodynamic control surfaces. The "fly by wire" technique was known already at that time, and was also applied to one of the two test aircraft used to develop all the electronic flight control techniques for the A-4. On the second aircraft, the control deflections were introduced thru a clutch system to the normal push rod linkages of the autopilot, which separated the hand controls in case of use of the autopilot. Both hydraulic and electric servomotors were used. By means of electric feedback of the control surface deflections and use of low inertia armatures (rotors) in the electric motors, the time constants of two types of electric servomotors could be reduced to four milliseconds, which was adequate to retain the control response frequencies above bending and torsional flutter modes of the missile structure.

Also at that time, magnetic amplifiers with feedback were developed that were uneffected by vibrations. They permitted, in several amplification stages, high amplification ratios, but avoided the sensitivity of electron tubes against acoustical and vibrational noise. A parallel effort to achieve this objective involved the semiconductor developments conducted by Professor Weise of the Technical University of Stuttgart, later the University of Michigan, that can be considered forerunners of the later transistors and diodes. I would go too far to describe all the individual efforts and solutions that advanced control and navigation techniques at that time, but among those who contributed were: the late Professor Leo Brand (1971+), also the late Professors Hueter, Kirschstein, Vieweg and Dr. H. Steuding, and also the still living Professors Buchhold, Busch, Fischel, Kramar, Carl Wagner and Wolman, as well as Dr. John Gievers, and my former collegues Dipl. Ing. Josef Boehm (+), Dipl. Ing. Karl Bruetzel (+), Dr. Kurt H. Debus, Dr. Hans Friedrich (+), Dr. Ernst Geisler, Dr. Reinhold Schubert, Dipl. Ing. Werner K. Gengelbach, Dr. Hans Gruene, Dr. Walter Haeussermann, Dr. Helmut Hoelzer, Dipl. Ing. Otto Hoberg, Dr. Joachim Muehlner, Dipl. Ing. Erich Neubert, Dr. Helmut Schlitt (+), Dipl. Ing. H. J. Gengelbach (+), Dr. Walter Schwidetzki, Dr. Ernst Stuhlinger and Dr. Hugo Woerdemann, a large number of whom became very prominent in the U.S. space effort, while others helped to usher in the European space effort. It was a true and remarkable effort.

All these men have to be recognized for their outstanding contributions and creative engineering achievements in their respective professional disciplines that included many pioneering engineering and scientific innovations of the highest caliber in the fields of flight control, inertial and radio guidance, mathematical modelling and simulation, and navigation technology. Many of these scientists continued their outstanding contributions to the science and technology of space flight into more recent times. On the American side, the progress achieved since World War II is based on the outstanding pioneering efforts of particularly Dr. Charles Stark Draper, Director of the MIT C.S. Draper Laboratories, and his close associates Prof. Walter Wrigley, Prof. R. H. Battin, Dr. David Hoag and the many outstanding members of this excellent Laboratory and others, who conceived and developed the extremely complex Apollo flight and navigation system.*

I would like to emphasize three particular technical solutions that have influenced even recent technical developments. In 1939-1940, Dr. Hoelzer, the Chief of the Flight Control and Guidance Division of the Peenemuende Rocket Research Center, developed a fully electronic analog computer that appears to have preceded all similar developments reported in the professional literature. Within a month it was applied to flight path simulation of ballistic missiles and ground to air missiles (the "Wasserfall" Anti Aircraft Guided Rocket), as well as a training device for joy-stick control of optically guided missiles. During the same period it was adapted to serve as the all electronic flight computer of the A-4, its main task. Two of these sets were converted to flight test units on two aircraft where responses to flight maneuvers were recorded. Based on the success of these units, we decided to use them as autopilots for these aircraft rather than the factory installed units. This decision led us next to superimpose on the autopilot inputs the signals of the radio guidance signals. Both applications turned out to be a great success.

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*See C. Stark Draper, "The Evolution of Aerospace Guidance Technology at the Massachusetts Institute of Technology, 1935-1951: A. Memoir," in this Volume—Ed.

The Kreiselgeraete Corporation developed a pendulous gyro accelerometer which integrated the acceleration produced by the thrust of the A-4 powerplant to the longitudinal velocity vector of the A-4. The numerical value of this accelerometer was used to cut off the powerplant at a predetermined value. Since the sea level thrust variation of these powerplants did affect the burnout velocity, it became necessary to apply burning time corrections to reduce the dispersion in the range of the ballistic missile. This was achieved by making a contact position of the velocity cut-off device variable with time, so that the integrator cut off at a lower velocity if it took longer to reach the preset speed. This integrator, a very simple device, showed in laboratory tests to be accurate in terms of a velocity uncertainty at burn-out, corresponding to .1% uncertainty in range dispersion, fuel valve dispersions included. Also, under flight conditions of the A-4, this device proved remarkably accurate.

A third invention by Professor Carl Wagner involved an electrolytic cell with silver-iodide serving as the electrolyte. It was used to integrate the electric current output of the Kreiselgeraete, Inc. and Buchhold accelerometers to the burnout velocity. Also, this cell was an extraordinarily simple integrator, remarkably accurate for the time period and the environment in which it was used. Prior to the takeoff, the cell was charged with a well defined small current, proportional to a small component of the gravitational field. By converting acceleration into a dc current we received an output proportional to the numerical value of the acceleration component. To reach the cutoff velocity specified in the firing tables, one just had to multiply the small component of the gravity to obtain the charging time necessary to store the cutoff-velocity in the cell, following the equation:

$$t = \frac{\nu_{\text{burnout}}}{o.1\ g} \qquad \text{where } \nu_{\text{burnout}} = 1650\ \text{msec}^{-1}\ g = 9.81\ \text{msec}^{-2}$$

$$t = \frac{1650}{o.981\ g} \qquad = \qquad 1682\ \text{sec. charging time}$$

Using a vertical dividing circle, accurate to one arcsec, a timing accuracy of .1 second, and a tilting angle of the accelerometer of 60.0 degrees ±1 arcsec, the acceleration would be integrated accurately to $\pm.422 \times 10^{-6}$ msec^{-2}. Kreiselgeraete Inc. developed air bearing levels which could measure the horizontal plane tilt accurate to one arc-second. Since automatic timing devices of much more than .1 second accuracy were common state of the art, the velocity uncertainty of the integration could be held to much lower values than indicated above. The charging process consisted of the electrolytic deposition of pure silver, removed from the electrolyte, on one of the electrodes. At take-off, the acceleration signal was simply reversed at the instant of lift-off and began to discharge the cell. At the instant of removal of all metallic silver from the cathode, the voltage across the terminals of the cell changed significantly and actuated the cut-off signal sequence. This integrator, except for the release, had no moving parts, was not sensitive to accelerations in the measuring direction, and proved remarkably reliable.

All power cutoff sequences at that time used a precutoff and a main cutoff command signal supplied by the flight control computer and its inertial subsystems. The precutoff signal closed the main fuel valves; a by-pass valve continued to furnish approximately 30% of the fuel, on account of residual drag at the altitude of cutoff and the direction of the flight path tangent at burnout, reducing the remaining acceleration to near zero. When the now slowly changing longitudinal velocity component reached the preset value, the main cutoff signal was activated, terminating all fuel flow. Of all the above quoted examples, the techniques introduced by Dr. Gievers and Dr. Hoelzer have found "permanent employment" in space technology. Thus, the technological advance of the state of the art emanating from the early ballistic missile developments at Peenemuende must occupy a prominent place in the history of rapid technological advancements of the twentieth century.

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PART IV

THE DEVELOPMENT OF LIQUID- AND SOLID-PROPELLANT ROCKETS, 1880-1945

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THE EVOLUTION OF AEROSPACE GUIDANCE TECHNOLOGY AT THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY
1935-1951: A MEMOIR[+]

C. Stark Draper[++]

INTRODUCTION

Guidance technology based on automatic, self-contained, onboard equipment for vehicles moving in six degrees of freedom with respect to the surface of the Earth has progressed from non-existence to practically its ultimate potential effectiveness over a span of not much more than three decades. Beginning in the early 1940s with the German development of the V-1 and V-2 missiles, and with other achievements in the USSR, the USA, and other countries, rapid strides in effective guidance for airplanes and spacecraft have been made with substantially universal acceptance of demonstrated principles a certainty as progress continues.

A strong interest in guidance, that complex of operating functions causing a directable vehicle to move along some path to accomplish its assigned mission, began for me in 1919 at the University of Missouri, when I went on my first airplane flight in an "OX5 JENNY." I transferred from Missouri and received a Stanford Bachelor's Degree in 1922. After graduation from the Massachusetts Institute of Technology in 1926 I enrolled in the Army Air Corps Reserve Officers Training Corps and attended flying school at Brooks Field in Texas. During the last years of the 1920s and early 1930s I owned and flew an OX5 ROBIN airplane. Even though the plane's performance was low, federal regulations were not yet in operation, so it was possible to fly and experiment at will in all kinds of weather. At this time I was on the instructing staff and later the faculty of the Department of Aeronautical Engineering at M.I.T., with free and complete access to measuring

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[+] Presented at the Fifth History Symposium of the International Academy of Astronautics, Brussels, Belgium, September 1971.

[++] Institute Professor Emeritus and President of the Charles Stark Draper Laboratory Division of the Massachusetts Institute of Technology. President of the International Academy of Astronautics.

instruments and machine tools. While teaching in aeronautics, I also worked toward a Doctor of Science Degree in Physics, with a Mathematics minor for specialization in advanced geometry and mechanics.

Because the professor who taught the courses in Aircraft Instruments left M.I.T., I was offered and accepted the assignment of teaching this subject. No systematic treatments of theory or textbooks of any kind were available. Consequently, there were no restrictions on formulating concepts, planning attacks, experiments, teaching patterns, instrument designs, and flight tests. This environment made it possible to identify, study, and find solutions for all phases of informetrics (the complex of activities dealing with the sensing, transmission, processing, evaluation and use of information) associated with the operation of aircraft. As an instructor in the Aeronautical Power Plant Laboratory, I worked under Professors C. F. Taylor and E. S. Taylor, and devised instruments for measuring engine pressures and vibration. This work attracted support from the National Advisory Committee for Aeronautics and the United States Navy, permitting me to start and maintain a small laboratory with a few assistants. These activities were established during the early years of the 1930s and in effect gave a start to the organization that eventually became the Charles Stark Draper Laboratory Division of the Massachusetts Institute of Technology.

PRELUDE FOR INERTIAL NAVIGATION SYSTEMS

Although the work with instrumentation during the 1930s was largely associated with aircraft engines, my strongest interest lay in formulating the problems associated with guidance, and designing equipment for meeting the requirements of operational situations. During and before the 1930s, radio aids for navigation were rudimentary, and, to make matters worse, owners of private airplanes usually could not afford receiving sets capable of satisfactory results. Weather reports were not generally available, and flight plans, except at the most important airports, were not required. The net result of all these circumstances meant that the private pilot could easily be caught in zero visibility, and reduced to his own resources without the possibility of help from ground stations or any other aircraft. Situations often developed in which good luck was required for survival, even if one abandoned all thought of mission accomplishment.

In several instances, a combination of poor weather and poor judgment left me with that "hopeless feeling," under conditions of substantially no visibility for seemingly very long periods of time. During these periods I was lucky to remain alive. However, after several bad experiences, and time to think over the consequences of events that occurred, I came to analyze the factors that combined to make safe flight impossible without visual contact with the Earth. The fundamental difficulty, of course, was the absence of geometrical information about the position, orientation and motion of the airplane with respect to the Earth. Two basic deficiencies were involved; first a clearly defined reference space in which orientation of the airplane could be judged, and second, a reference space, which might be the same one as that providing orientational references, from which direction of travel and linear speed of the aircraft with respect to the Earth could be estimated. Orientation, that is, roll and pitch about "horizontal" axes, and yaw, the angle from north, were needed for maintaining stabilization (angular positions about three axes well enough defined for the maintenance of safe flight while providing reference directions from which maneuvering changes in attitude and direction can be made), while the position, direction, and speed over the Earth were essential knowledge for navigation toward selected destinations.

Orientational references for the purposes of flight stabilization were first provided in practical flight instruments in 1928 with the Artificial Horizon and Directional Gyro developed by Elmer A. Sperry, Jr., for the Guggenheim Blind Flying Experiments of James H. Doolittle. The instruments gave indications showing aircraft deviations in roll, pitch, and yaw, read from dials by the pilot. With these indicated deviations, the pilot could stabilize the aircraft through his usual control movements just as he would have applied information from visual observations of the Earth's surface on clear day. The Sperry instruments were for stabilization only, and provided angular outputs good to a few degrees of angle; rough outputs by navigational standards, but adequate for maintaining control and reasonably good flight directions for human pilots. The Sperry devices did not indicate the position or velocity of the aircraft with respect to the Earth, and thus offered no direct help with navigation.

To be sure, some discussion of determining the position changes of aircraft by double integration of indicated accelerations did indeed take place among aeronautical engineers during the early 1930s. But recognition of the high accuracy required and the need to separate effects due to gravitational fields from inertial reaction forces, led to the near-universal conclusion that the development of necessary instrumentation would involve very great difficulties. Therefore, most engineers and designers decried any attempt to develop self-contained guidance systems based entirely on Newton's Principles of Inertia as a waste of time and money.

After some fifteen years experience and study of the theoretical and engineering problems associated with creating inertial guidance equipment, I came to disagree with this conclusion. At the same time, however, I also became convinced that creating practical means for guidance of this kind would be very, very difficult. Familiarity with state-of-the-art technology made me certain that "off-the-shelf" devices offered no real help with design or construction. Everything, the elements, components, and subsystems, would have to be imagined and created from theory, engineered, built, tested, prepared for production and operational service "starting from scratch." Substantial support for several years would be necessary if significant results were to be achieved. But support of this kind was simply unavailable during the 1930s. Consequently, beyond a few thesis projects, studies in practical "blind flight" at the Boeing School of Aeronautics in Oakland, California, some flight tests of conventional aircraft instruments, and slow but careful development of theory in mechanics with particular stress on gyroscopics incorporated into graduate student subject matter, I placed inertial guidance developments on an inactive status until more favorable conditions might appear.

For self-contained guidance systems, the fundamental problems involved using instruments to accurately indicate changes in six independent geometrical quantities, three rotations about mutually orthogonal axes, and three linear translations along these or three other mutually orthogonal axes. The general performance objectives for measuring devices to deal with the geometrical quantities essential for inertial guidance had to fall into a pattern similar to the typical behavior of watches as indicators of elapsed times after the arbitrarily selected initial instants. In addition to measurements of this kind--for changes in three angles and three translations from configurations chosen generally for convenience--indications of velocity (rate of change of position) and acceleration (rate of change of velocity) as direct or derived signals were also required for guidance system outputs.

Just as specific uses determine the performance requirements for watches, the use made of geometrical quantity sensors would set the quality of results desired from overall systems. For example, good watches may accumulate errors at rates no more than fractional seconds per day, while quartz crystal or atomic oscillators can be built to realize accuracies in the range of one part in 10^{10} and better. This wide spectrum of performance in measurements of time required many years of development before it became a matter of practice. Likewise, one could reasonably expect that some years would be needed to bring sensors for inertial quantities to corresponding levels of performance.

The performance required depends upon the following circumstances existing on our planet. On the Earth's surface, one minute of arc angle between local vertical directions means that a distance of one nautical mile (6,000 feet) has been passed on the Earth's surface. The corresponding distance for one second of arc (1/60 of a minute) is 1/60 of a mile, so that a deviation in alignment of reference coordinates to the vertical of one second in magnitude would produce a navigational indication error of one hundred feet. Therefore, the angular reference coordinates carried by an airplane had to have angular uncertainties with respect to the Earth not greater than a few seconds of arc about horizontal axes if this subsystem was to give accurate indications within a few hundred feet.

Instruments mounted on stabilized members to sense acceleration components for navigation computations would, in practice, actually sense not acceleration alone, but "specific force," the resultant of gravitational field action and inertial reaction. The signals representing specific force components would have to be "corrected" for gravitational field effects in the computing system that would also provide single and double integrations to produce indications of changes in velocity and position. If errors in position on the Earth's surface were to be limited to not more than a few hundred feet during a flying time of about one hour, specific force receivers had to have performance characteristics that allowed them to deal reliably with changes on the order of one ten thousandth or less of one Earth's gravity. I conducted various mathematical studies of the performance needed for inertial quantity sensors to serve as components for on-board aircraft guidance systems with my colleagues, notably Professor Walter Wrigley and his students during the last half of the 1930s and the first half of the 1940s, and by 1945 the instrument performance characteristics required for inertial systems were generally well recognized.

Under the impetus of World War II, radio aids for navigation received great attention and support. By war's end, civilian and military airplanes carried equipment that enabled pilots to navigate quite well under all weather conditions. Of course, positive results depended upon the cooperation of favorable radiation environments from extensive ground station networks designed to complement onboard radio equipment. However, I still remembered my days of flying without external aids and retained a strong personal interest in the challenge associated with creating self-contained, on-board guidance systems not requiring radiation contacts with external stations or points of the Earth.

Inertial guidance system developments received no significant support in our Laboratory during the early 1940s. But enemy control of vast territories during World War II soon stimulated funding of self-contained on-board equipment for aircraft guidance. Because such systems did not require help from outside stations and could not be put out of action by any measures short of actual physical destruction, World War II experiences caused Air Force officials to consider development of inertial guidance systems as essential for future capabilities. Late in 1945 Colonel (now Lieutenant General, retired) L. I. Davis, the Commandant of the Armament Laboratory, who had been my graduate student, and his chief scientist, Dr. J. E. Clemens, translated their understanding of inertial guidance possibilities into support for our Laboratory. Shortly thereafter, we began a project to design and build an experimental system. The following sections of this paper consider this work and the progress achieved during the period ending in 1951.

ACTIVITIES DURING THE FIRST HALF OF THE 1940s

Aerospace guidance was not an area that provided sponsors for Laboratory activities during the first half of the 1940s. In fact, during the last half of the 1930s the Laboratory was largely concerned with aeronautical power plant instrument projects—particularly with creating engine analyzers for long flights over water. Graduate courses on instrumentation that I taught attracted a number of Navy and Air Corps officers in addition to civilian students and, until 1939, I continued to be especially interested in the generalized geometry associated with aircraft motions and also the consequences of Newton's Laws for linear and rotational motions of rigid bodies. Progress in these areas came from preparations for lectures, laboratory work, and thesis projects associated with educational activities of the Laboratory which was then, as it always has been during its existence, an integral part of the academic section of the M.I.T. Aeronautical Engineering Department.

During the early phases of World War II, multiple air strikes at surface vessels had sad results for ships of the Allied Navies. At this time M.I.T. students, faculty, and members of the Instrumentation Laboratory became strongly concerned with developments of equipment to provide protection against such attacks. Existing guns and their associated fire control equipment had been designed for battles among surface vessels and were so large and heavy that only a small number of systems could be installed aboard even the largest ships. These gun projectiles were larger than necessary to destroy airplanes with direct hits or even near misses, while their rates of firing were slow. The equipment available for pointing guns depended on cumbersome mechanical arrangements for geometrical transformations between tracking coordinates, computing coordinates, and finally the deck coordinates in which gun movements had to occur. The control systems were not only large and heavy, but were so sluggish in action that the airplane's relatively high speed made it possible for them to complete their missions and fly safely away without much danger from anti-aircraft shells.

Designs for rapid acting machine guns firing projectiles capable of destroying airplanes by direct hits were available, with large scale production only a matter of assigning adequate resources to the task. But equipment for rapidly and effectively pointing these guns was another matter entirely, because the bulk, complexity, and cost of existing devices, even if they had been effective, made it impossible to provide control for each gun or group of guns. The general approach used for the gun design was not adaptable for reductions in weight, size, complexity, or significantly lowered costs. Consequently, during 1940, we directed our attention toward this problem of providing rapid and effective anti-aircraft fire control for machine guns carried by naval ships. Because we became involved almost exclusively with classified projects, the Laboratory was now given the new name of CONFIDENTIAL INSTRUMENTS LABORATORY.

The engineering problems involved developing fire control units (1) weighing not more than a few tens of pounds, (2) able to operate satisfactorily while mounted directly on the cradle of a firing machine gun, (3) requiring no more than a few minutes training time for normal human beings to become competent operators, (4) having good reliability, and (5) priced at not more than a small fraction of the cost for conventional fire control equipment. The capabilities for providing gunsights were based on adaptations of models already built earlier. The success of our concepts and equipment, designed and produced by the Sperry Gyroscope Company and associated contractors, was clearly demonstrated during many ship and aircraft encounters during World War II. This achievement brought the Laboratory, now renamed the INSTRUMENTATION LABORATORY, wide recognition. It also generated the background of ideas, cadres of people, and equipment necessary for assuming tasks in guidance based on inertial principles.

On the engineering side, the development of naval antiaircraft fire control systems and airborne equipment for fighters and bombers permitted us to create and reduce to practice several unconventional viewpoints toward dynamical theory. Later these viewpoints became key factors in designs for the basic components of inertial systems. A breif look at the principles applied is not only useful but essential for understanding the fundamental background in discussions of later developments. The theoretical background is based on Newton's Law of Inertia which states that the vector change in momentum of a mass with respect to inertial space is determined by the applied force, being in the direction of the force, proportional in magnitude to the magnitude of the force, and inversely proportional in magnitude to the magnitude of the mass involved; applied to rotational motion, this law becomes the statement that the time rate of change of angular momentum of a body with respect to inertial space is equal to the applied torque.

Students of mechanics are well aware that for bodies of generalized shapes with forces and torques arbitrarily applied, the mathematical expressions for the resulting motions are complex, and descriptions of behavior quite involved. Years of study had brought me considerable familiarity with the exhaustive treatments found in comprehensive textbooks. But practical experience had convinced me that, for engineering purposes, all the essential actions of sensing devices could be derived from theoretical assumptions of relatively simple mechanical arrangements. The basic ideas were elementary, and corresponded to design parameters that could be adapted to practical constructions by simplifying the six-degree-of-freedom circumstances of a body free in space in situations where essential actions could be effectively described by considering only one or two degrees of freedom. This implied designs of constraint systems for unbalanced masses or spinning rotors that restricted motions to conical angles with respect to a point or to simple angular displacements about a single axis of rotation.

Before the early 1940s, when the Laboratory developed antiaircraft gunsights for operation on the moving decks of surface vessels, the gyroscopic rotors commonly used in marine compasses and naval stabilization equipment were carried by arrangements having two degrees of rotational freedom. The suspensions employed elastic members and bearings designed to work with very small friction levels. It followed that, ideally, the supporting means caused negligibly small uncertainty torque components to act on the angular momentum associated with the spinning rotors. Under Newton's Law for Gyroscopic Action, this meant that, because "zero" torque was acting, the angular momentum vector did not change its direction with respect to inertial space, consequently, the spin axis exhibited the property of being "rigid in space." Instrument operation using gyroscopic rotors generally depended upon this action for stabilization to exclude effects of undesired base motions, while using changes in spin axis direction to produce control torque components for generating desired results. Two-degree-of-freedom gyro units, having suspensions without significant friction uncertainties, were said to have the basic property of "rigidity-in-space," while essential operation was associated with precession under torque components derived from control inputs within associated mechanisms.

All of the fire control developments at the Laboratory were based on single-degree-of-freedom gyro units instead of the two-degree-of-freedom type that was general for both marine and aircraft applications before 1940. The only instrument in common use before 1940 that employed the single-degree-of-freedom design feature was the so-called Rate-of-Turn Indicator for aircraft, that applied the ideas from patents of Professor Henderson. This device did not and was never intended to provide performance of quality suitable for navigation or guidance.

In the machine gun fire control equipment developed by the Laboratory, three single-degree-of-freedom gyro units were mounted on a rigid member with the spin axes of their rotors mutually orthogonal, and carried by single axis gimbals with directions of their rotational freedom at right angles to each other. Variable spring restraints about the gimbal axes were adjustable in stiffness as a function of range to the target. Viscous action of a thick fluid in the clearance between the gimbal and the case was used to provide protection against mechanical shock and vibration, while at the same time supplying drag torque to damp out the effects of roughness associated with gun shock and vibration. As the gunner moved his weapon, rotation of the member carrying the gyros caused the three gimbals to tip until the restraining springs developed restraining torques to balance the gyro output torques corresponding to components of input angular velocity.

These gimbal rotations were coupled mechanically to a system of mirrors whose angular deflections established on optical-reticle indicated line of sight offset behind the gun barrel. The gunner had only to keep moving the gun as necessary to maintain the reticle image on the target. The corresponding forced motion of the gunsight case generated lead angles that caused the gun to fire its projectiles ahead of the target by a proper angle to correct for target motion during bullet travel. With this arrangement, the fire control problem was effectively solved using inertial space, and it was unnecessary to mechanize any coordinate transformations. In effect, deck motions did not enter the problem, being eliminated by a gunner who kept the reticle on the target, so that they did not affect the fire control predictions.

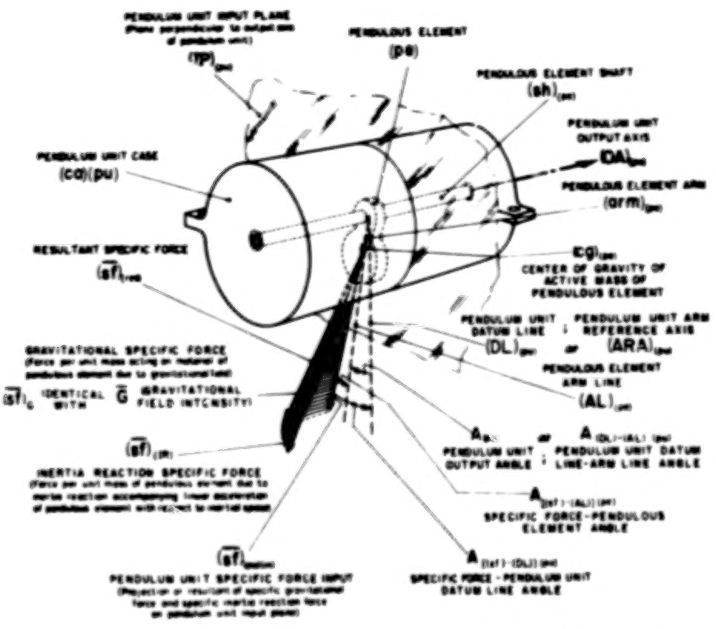
In practice, this was not altogether true because human gunners were unable to accommodate perfectly for deck tilts and angular velocities to keep his optics exactly on the target, but the mechanization allowed inexperienced gunners to deliver effective anti-aircraft fire under combat conditions. Since the fire control problem did not need geometrical transformation devices, the gunsights were compact, being not much larger or heavier than standard typewriters, while they generated effective solutions rapidly enough to keep attacking aircraft in considerable danger while they remained within range of the guns. Though the fire control system designs were not important for the inertial navigation systems, they applied single-degree-of-freedom gyro units and provided an extensive and fundamental engineering and design background for the sensors that would be developed for Inertial Navigation Systems.

INERTIAL SENSOR PRINCIPLES: FUNDAMENTALS OF GYROSCOPIC THEORY

Aerospace guidance technology developed by the Laboratory depended upon engineering applications of Newton's Laws. These applications are difficult to interpret without a pattern of generalized theory to simplify concepts and clarify representations. A pattern of this kind is developed in Figures 1 through 6. Devices of two types are important for inertial systems; one type, called the specific force sensor, is required to be responsive for resultants formed by gravitational and inertial reaction forces which act on each particle of the materials involved. The other type, called the angular deviation sensor, receives angular deviations with respect to inertial space from arbitrarily established reference orientations. Sensors of the first type generate signals from which navigational information on acceleration, velocity, and location may be derived. Sensors of the second type supply signals that can be used as inputs to servo-drive systems for correcting deviations of specific force sensors from desired orientations.

Figure 1 is taken from an Instrumentation Laboratory publication of the 1940s showing the essential sensing element in a single-degree-of-freedom specific force receiver consisting of an unbalanced mass carried by a shaft, pivoted about an axis fixed to the instrument case. In practice, damping, output signal generation, torque restraint, and other functions have to be included as necessary services provided for the force-sensing mass in a complete instrument. Some of the various essential components that must be combined to give overall operation are labeled and named.

Gyroscopic actions associated with a rotor spinning at relatively high angular velocity about an axis of symmetry are generally regarded as less obvious than the apparently straightforward dynamic "lagging-behind" of a mass under linear acceleration. In fact, if vector representations are understood and used, smooth, high-speed rotation has the effect of one integration, and makes the behavior of a gyro constrained to single-degree-of-freedom operation even simpler than the interaction of a mass with linear acceleration. Figure 2 is a summary of the definitions, conventions, and representations used to associate gyro rotors with vectors and torques. Figure 3 is a pictorial representation of a gyro rotor and gimbal mounted with two degrees of angular freedom on


PENDULUM UNIT INPUT PLANE
(ip)(pu)
PENDULOUS ELEMENT
(pe)
PENDULOUS ELEMENT SHAFT
(sh)(pe)
PENDULUM UNIT OUTPUT AXIS
(OA)(pu)
PENDULUM UNIT CASE
(ca)(pu)
PENDULOUS ELEMENT ARM
(arm)(pe)
RESULTANT SPECIFIC FORCE
(sf)(res)
(cg)(pe)
CENTER OF GRAVITY OF ACTIVE MASS OF PENDULOUS ELEMENT
PENDULUM UNIT DATUM LINE
PENDULUM UNIT ARM REFERENCE AXIS
(DL)(pu) or (ARA)(pu)
GRAVITATIONAL SPECIFIC FORCE
PENDULOUS ELEMENT ARM LINE
(AL)(pe)
(sf)G IDENTICAL WITH G (GRAVITATIONAL FIELD INTENSITY)
(sf)(IR)
A or A(DL)-(AL)(pu)
PENDULUM UNIT OUTPUT ANGLE
PENDULUM UNIT DATUM LINE-ARM LINE ANGLE
INERTIA REACTION SPECIFIC FORCE
A[(sf)-(AL)](pe)
SPECIFIC FORCE-PENDULOUS ELEMENT ANGLE
(sf)(input)
PENDULUM UNIT SPECIFIC FORCE INPUT
A[(sf)-(DL)](pu)
SPECIFIC FORCE - PENDULUM UNIT DATUM LINE ANGLE

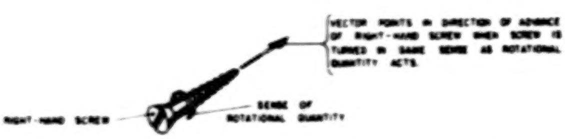
NOTE IN PRACTICE, THE UNIT MUST INCLUDE A SIGNAL GENERATOR OF SOME TYPE TO RECEIVE THE PENDULUM UNIT OUTPUT ANGLE AND PRODUCE AN OUTPUT SIGNAL REPRESENTING THE ANGLE

THE SUBSCRIPT FOR THE SYMBOL A, WHICH REPRESENTS AN ANGLE, IS A DUAL SYMBOL ENCLOSED IN BRACKETS THE FIRST SYMBOL IS ASSOCIATED WITH THE REFERENCE DIRECTION, THE SECOND SYMBOL IS ASSOCIATED WITH THE LOCATED DIRECTION.

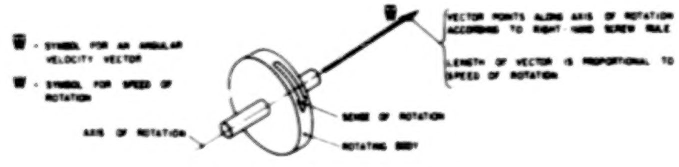
Fig. 1
Line Schematic Diagram Showing Single-Degree-of-Freedom Pendulum Unit as an Orientational Signal Receiver for the Direction of Specific Force Projected on an Input Plane

a base. With the spin axis substantially horizontal, a force applied to the inner gimbal about the axis at right angles to the gimbal axis produces a torque vector at right angles to the angular momentum vector. By Newton's Law of Gyroscopic Action, the response of the rotor is to turn its spin axis toward the torque axis in the angular motion called precession. The direction of change of position always tends to shift the arrowhead of the moving vector so that it points in the same direction as that of the torque vector. The diagram of Figure 3 is, of course, greatly simplified, in single-degree-of-freedom gyro units for use as angular deviation sensors in guidance systems.

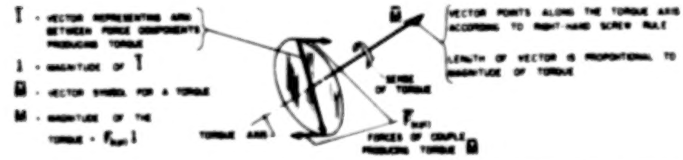
Figure 4 summarizes the engineering definitions made in treating spinning rotors as gyroscopic elements, and the theoretical consequences that follow these assumptions. By definition, a gyroscopic element includes the following three features: (1) a rotor spinning about an axis of symmetry, (2) the spin angular velocity is constant in magnitude, and (3) angular velocity magnitudes about axes other than the spin axis never exceed insignificant levels, and moments of inertia about axes at right angles to the spin axis are all so small that the angular momentum of the system is effectively concentrated in the rotor along the spin axis.


VECTOR POINTS IN DIRECTION OF ADVANCE OF RIGHT-HAND SCREW WHEN SCREW IS TURNED IN SAME SENSE AS ROTATIONAL QUANTITY ACTS.
RIGHT-HAND SCREW
SENSE OF ROTATIONAL QUANTITY

a) Right-hand screw rule for the relationship between a rotational quantity and the vector representing this quantity

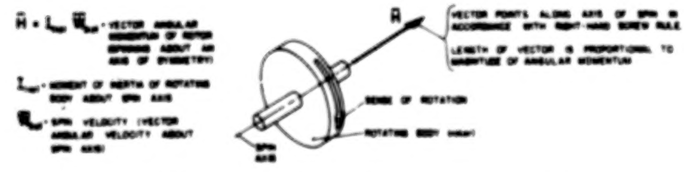

VECTOR POINTS ALONG AXIS OF ROTATION ACCORDING TO RIGHT-HAND SCREW RULE
LENGTH OF VECTOR IS PROPORTIONAL TO SPEED OF ROTATION
- SYMBOL FOR AN ANGULAR VELOCITY VECTOR
- SYMBOL FOR SPEED OF ROTATION
AXIS OF ROTATION
SENSE OF ROTATION
ROTATING BODY

b) Vector representation of angular velocity


- VECTOR REPRESENTING ARM BETWEEN FORCE COMPONENTS PRODUCING TORQUE
- MAGNITUDE OF
- VECTOR SYMBOL FOR A TORQUE
- MAGNITUDE OF THE TORQUE
VECTOR POINTS ALONG THE TORQUE AXIS ACCORDING TO RIGHT-HAND SCREW RULE
LENGTH OF VECTOR IS PROPORTIONAL TO MAGNITUDE OF TORQUE
SENSE OF TORQUE
TORQUE AXIS
FORCES OF COUPLE PRODUCING TORQUE

FOR THIS DIAGRAM THE TERM "TORQUE" HAS A MEANING IDENTICAL WITH THE WORD "COUPLE" AS USED IN CONVENTIONAL MECHANICS

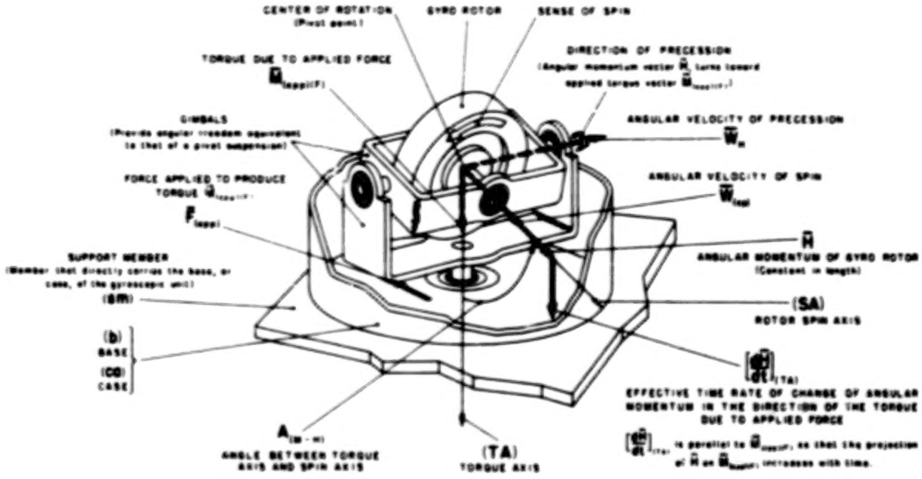
c) Vector representation of a torque


- VECTOR ANGULAR MOMENTUM OF ROTOR SPINNING ABOUT AN AXIS OF SYMMETRY)
- MOMENT OF INERTIA OF ROTATING BODY ABOUT SPIN AXIS
- SPIN VELOCITY (VECTOR ANGULAR VELOCITY ABOUT SPIN AXIS)
VECTOR POINTS ALONG AXIS OF SPIN IN ACCORDANCE WITH RIGHT-HAND SCREW RULE
LENGTH OF VECTOR IS PROPORTIONAL TO MAGNITUDE OF ANGULAR MOMENTUM
SENSE OF ROTATION
ROTATING BODY (rotor)
SPIN AXIS

d) Vector representation for the angular momentum of a spinning rotor

Fig. 2
Vector Representation of Rotational Quantities

Under these assumptions, the equation of motion for a gyroscopic element reduces to the statement that the applied torque is equal to the cross product of the angular velocity of the spin axis with respect to inertial space and the angular momentum vector. In other words, the angular momentum vector has an angular velocity of precession that turns it toward the torque vector. The output torque from the gimbal when an angular velocity with respect to inertial space is imposed on the angular momentum vector, is given by reversing the order of terms in the cross product. Figure 5 illustrates an arrangement with a gyroscopic element incorporated in an instrument case as the sensitive element of a basically single-degree-of-freedom angular deviation receiver. The vector component performance equations are written down for a generalized orientation of the gyro element and its gimbal within the case. In engineering practice, all the angles between axes fixed to the structure carrying the rotor and the enclosing case remain very small, so that small angle assumptions in theory are all valid for all deviations as they are shown.. Figure 6 is a line schematic diagram, with definitions and symbols, showing a gyro

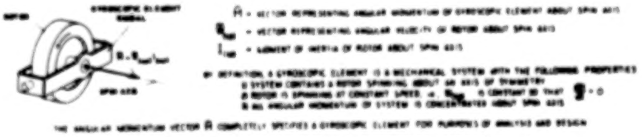

CENTER OF ROTATION
(Pivot point)
GYRO ROTOR
SENSE OF SPIN
TORQUE DUE TO APPLIED FORCE
$\bar{M}_{(appl)(F)}$
DIRECTION OF PRECESSION
(Angular momentum vector $\bar{H}$ turns toward applied torque vector $\bar{M}_{(appl)(F)}$)
GIMBALS
(Provide angular freedom equivalent to that of a pivot suspension)
ANGULAR VELOCITY OF PRECESSION
$\bar{W}_{H}$
FORCE APPLIED TO PRODUCE TORQUE $\bar{M}_{(appl)(F)}$
$\bar{F}_{(appl)}$
ANGULAR VELOCITY OF SPIN
$\bar{W}_{(sp)}$
$\bar{H}$
ANGULAR MOMENTUM OF GYRO ROTOR
(Constant in length)
SUPPORT MEMBER
(Member that directly carries the base, or case, of the gyroscopic unit)
(SM)
(SA)
ROTOR SPIN AXIS
(b)
BASE
(CO)
CASE
$\left[\frac{d\bar{H}}{dt}\right]_{(TA)}$
EFFECTIVE TIME RATE OF CHANGE OF ANGULAR MOMENTUM IN THE DIRECTION OF THE TORQUE DUE TO APPLIED FORCE
$\left[\frac{d\bar{H}}{dt}\right]_{(TA)}$ is parallel to $\bar{M}_{(appl)(F)}$ so that the projection of $\bar{H}$ on $\bar{M}_{(appl)(F)}$ increases with time.
$A_{(M-H)}$
ANGLE BETWEEN TORQUE AXIS AND SPIN AXIS
(TA)
TORQUE AXIS

The effect of the component of the time rate of change of the angular momentum at right angles to direction of the torque due to the applied force shown is disregarded in the diagram. In order to keep the direction of the resultant applied torque perpendicular to the angular momentum vector, an additional vector torque component must be applied to the gimbal system by the support member

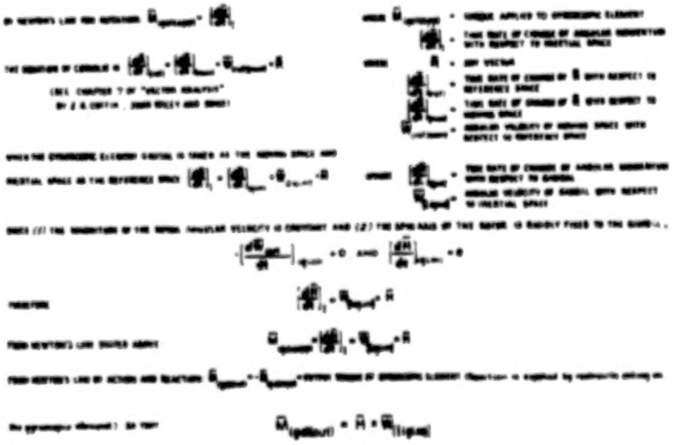
All angular velocities and rates of change are with respect to inertial space.

BY DEFINITION, PRECESSION IS THE TURNING OF THE ANGULAR MOMENTUM VECTOR TOWARD THE APPLIED TORQUE VECTOR

Fig. 3
Pictorial Schematic Diagram of Gyroscopic Unit Model Illustrating the Precession of a Gyroscopic Element Due to an Applied Torque Acting About a Fixed Axis



(a) VECTOR REPRESENTATION OF GYROSCOPIC ELEMENT



(b) PERFORMANCE EQUATION FOR GYROSCOPIC ELEMENT

Fig. 4
Vector Representation and Basic Performance Equation for the Gyroscopic Element

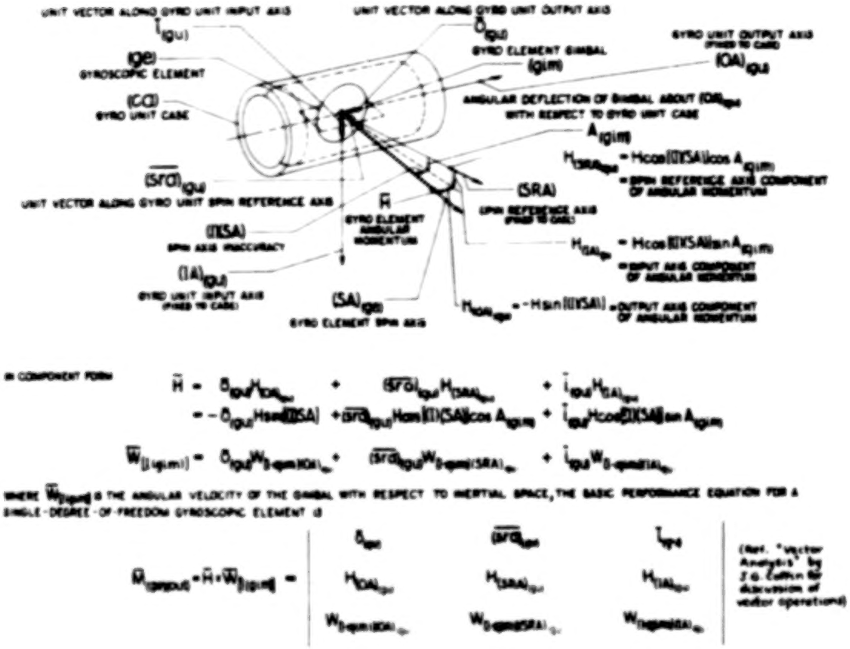

UNIT VECTOR ALONG GYRO UNIT INPUT AXIS
UNIT VECTOR ALONG GYRO UNIT OUTPUT AXIS
(ge)
GYROSCOPIC ELEMENT
GYRO ELEMENT GIMBAL
(gim)
GYRO UNIT OUTPUT AXIS (FIXED TO CASE)
(OA)
(CC)
GYRO UNIT CASE
ANGULAR DEFLECTION OF GIMBAL ABOUT (OA) WITH RESPECT TO GYRO UNIT CASE
A(gim)
= SPIN REFERENCE AXIS COMPONENT OF ANGULAR MOMENTUM
(SRA)
SPIN REFERENCE AXIS (FIXED TO CASE)
UNIT VECTOR ALONG GYRO UNIT SPIN REFERENCE AXIS
GYRO ELEMENT ANGULAR MOMENTUM
SPIN AXIS INACCURACY
= INPUT AXIS COMPONENT OF ANGULAR MOMENTUM
(IA)
GYRO UNIT INPUT AXIS (FIXED TO CASE)
(SA)
GYRO ELEMENT SPIN AXIS
= OUTPUT AXIS COMPONENT OF ANGULAR MOMENTUM
IN COMPONENT FORM
(Ref. "Vector Analysis" by J. G. Coffin for discussion of vector operations)

Fig. 5
Coordinate Diagram and Static Performance Characteristic for
Single-Degree-of-Freedom Gyro Unit with Non-Ideal Geometry

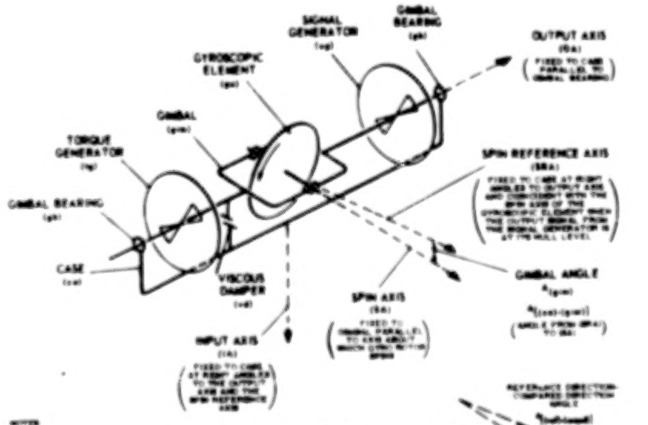

SIGNAL GENERATOR (sg)
GIMBAL BEARING (gb)
OUTPUT AXIS (OA)
GYROSCOPIC ELEMENT (ge)
GIMBAL (gim)
TORQUE GENERATOR (tg)
SPIN REFERENCE AXIS (SRA)
GIMBAL BEARING (gb)
CASE (ca)
VISCOUS DAMPER (vd)
GIMBAL ANGLE
SPIN AXIS (SA)
INPUT AXIS (IA)
NOTES
REFERENCE DIRECTION
COMPARED DIRECTION
CASE
GYRO UNIT
TORQUE GENERATOR
SIGNAL GENERATOR
DAMPER
GIMBAL

Fig. 6
Line Schematic Diagram for the Single-Axis Integrating Gyro Unit (Single-Degree-of-Freedom Angular Deviation and Command Signal Receiver)

element combined with the other functional components needed to form the fundamentals of a single-degree-of-freedom angular deviation receiver that is adequate for engineering discussion, design and test purposes.

The combination of concepts and components defined and represented in simple form by the line schematic diagram of Figure 6 forms the basis for all the single-degree-of-freedom integrating gyro units created and pioneered during the 1940s by the Instrumentation Laboratory. Angular deviation sensors based upon the features summarized in Figure 6 have been manufactured in large numbers and successfully applied in many aircraft, spacecraft and marine systems. Sensors of the type illustrated derive their name from the fact that rotation of the case about the input axis can only occur if a torque exists which, imposed upon the rotor, causes the angular momentum vector to turn toward the torque vector about the output axis. This turning is resisted by a viscous drag torque in the damping fluid which is proportional to the angular velocity of the damper within the case. Since this torque is proportional to the "output" angular velocity of the gimbal about the gimbal axis, which in turn depends on the gyroscopic torque produced by the "input" angular velocity, the overall action is one of integration. The electrical output from the signal generator measuring the rotation of the gimbal within the case, for this reason gives a direct indication of rotation for the case with respect to inertial space about the input axis. Many refinements and complexities beyond the elements suggested in Figure 6 are necessary before working sensors can be realized, but the fundamental ideas involved are described by the diagrams. Because the basic gyroscopic input is case angular velocity about the input axis, while the unit output is an electrical signal representing the integral of this input, instruments with the features represented in Figure 6 are called Single-Degree-of-Freedom Integrating Gyro Units, or more usually, IRIG's (Inertial Reference Integrating Gyros).

STATE OF INERTIAL EQUIPMENT ART IN THE LATE 1940s

At the end of 1945, changed circumstances made it possible for the Instrumentation Laboratory to actively attack the problem of inertial control, navigation, and guidance for aircraft. At that time the only effective devices that used inertial and gravitational effects associated with the Earth were the marine gyro compass and the stable unit used for fire control reference purposes. The rotors in these instruments all had two degrees of freedom and weighed many pounds. The complete equipments stood some four feet high with horizontal dimensions approximating one foot. Many engineers believed that large, rapidly spinning wheels were the only means for sensing the Earth's rotation and detecting deviations from reference orientations small enough to provide practical stabilization. In common with all government sponsors of new projects, the Air Force wanted to make use of existing technology for airborne inertial systems.

Following this plan of attack, the Laboratory installed and flight tested an ARMA Stable Vertical Model (commonly applied in the U.S. Navy for fire control purposes) in an Air Force DC-2. The equipment was large and heavy. Without outside reference, vertical indication performance was not good enough for airborne navigation and guidance aimed at errors less than one mile at the end of a ten-hour flight period. It has already been noted that the aircraft Bank and Climb Indicator and the Turn Indicator had performance limitations with the order of a few degrees which, in the 1940s (and for all later times), qualified them for stabilization service but not for navigation or guidance. At the beginnings of the Instrumentation Laboratory's work on inertial navigation and guidance, we concluded that new mechanization would have to be developed from fundamental principals without dependence on anything available from existing technology.

FUNDAMENTAL MECHANIZATION REQUIREMENTS FOR CONTROL, NAVIGATION, AND GUIDANCE SYSTEMS

Achieving practical results without undue concern for the disciplines in science and engineering became my early pattern of action at M.I.T. Following this approach, the Instrumentation Laboratory became a laboratory of pioneering technology. Thus, the technology represented by the 1946 Air Force Project involved searching out all the necessary inputs and generating the control and guidance commands needed to stabilize aircraft motions and determine flight paths. We clearly recognized that the overall processes had to deal with the quantities of generalized six dimensional geometry, and that self-contained systems for the sub-function of navigation could not depend upon continuous radiation contacts with known points of the Earth. This meant furnishing, on-board, coordinates stabilized against erratic and systematic aircraft rotations, suitably arranged to maintain the input axis of specific force sensors so that all possible resultants of gravitational forces and linear accelerations were completely accepted.

Flight tests of gyroscopic units used for marine applications had clearly demonstrated that such equipment was not only deficient in performance, but had sizes and weights beyond those allowable for aircraft. To overcome these objections, we decided on design studies to minimize the weight and bulk of gyro rotors and their supporting gimbals, and to reduce levels of undesired torque acting to cause erratic angular deviations of the active angular momentum vector. This meant abandoning the concept of a heavy rotor--holding its spin axis direction in the presence of torque by sheer inherent power of spinning mass--for a mechanization where gyro units, using relatively small rotors carefully isolated from undesired torque components, acted to operate signal generators requiring substantially zero torque with outputs representing angular deviations. In

1945, the relatively new technology of servomechanisms was perceived as the key to converting gyro deviation signals into torque components for maintaining geometrical members in reference orientations substantially free of interference by supporting base motions.

Thus, without concern for details of design, the functions needed to implement geometrical reference functions were recognized as:

1) One or more mechanical reference members with a total of three degrees of angular freedom with respect to the supporting aircraft.

2) Angular deviation sensors to detect orientational changes of the reference member from desired reference positions and to generate corresponding output signals.

    a) Two two-degree-of-freedom angular deviation sensors.

    b) Three single-degree-of-freedom angular deviation sensors.

3) Servodrive systems to accept the angular deviation sensor signals and apply torque components to the geometrical reference member supports as required to overcome the friction, unbalance, and other torque components tending to disturb the geometrical member orientations from desired reference positions.

4) Means incorporated in the gyro units to sense angular deviations for supplying servodrive inputs to cause changes of the geometrical reference member orientations in response to command inputs.

5) Angle sensors to provide signals representing aircraft orientation with respect to the geometrical reference member.

6) Specific Force Sensors with their input axes properly related to known directions in the geometrical reference member so that the output signals representing components along their input axes could be combined to provide complete information on the resultant specific force input.

7) Electronic components as necessary to service stabilization drives and sensors.

8) Computing systems:

    a) To receive signals representing orientation of the geometrical reference member with respect to Earth or directions determined by celestial bodies and generate orientational change commands for the reference member.

    b) To receive signals from the specific force sensors, correct out gravitational effects and generate indications of location and velocity with respect to the Earth.

    c) To compare indicated locations and velocities with respect to programmed states of these quantities in terms of Earth or other chosen external spaces, and from this comparison to determine the corrections to vehicle motion needed to achieve final mission success.

    d) To generate command signals to serve as guidance system outputs representing these corrections and to serve as the essential inputs for the control system which provides interface functions to couple the guidance system with the vehicle and its driving system.

9) Various readouts, indicators, monitoring arrangements, mode-of-operation selectors, manual controls, etc., to meet the requirements of particular situations.

Even a brief discussion of the subjects suggested in this far-from-detailed listing is impossible within the scope of this paper. There is surely no reason to review here, the theory, mathematics, and practice of the ancient art of navigation available in 1946. Similarly, the computer functions required and state-of-the-art components available for use in new equipment were well known. So much effort elsewhere was being devoted toward progress that the Instrumentation Laboratory found no valid excuse for adding any new workers to the fields of computer technology. Considerations of the same kind led the Laboratory away from tasks associated with readout arrangements and indicators. It appeared that the area where the Laboratory could make truly significant contributions included sensors for inertial quantities, and mechanizations associated with the realization of geometrical reference members to achieve the objective of an error build-up of one mile in ten hours flight by a self-contained guidance system. Knowing that all the fundamental components had to be conceived, designed, built and tested, this was the goal the Laboratory began to work toward in 1946.

GEOMETRICAL REFERENCE MEMBER MECHANIZATION: PRINCIPLES

Inertial system work in the Laboratory started with design studies following the general two-degree-of-freedom gimbal suspension pattern that was commonly found in gyroscopic equipment for marine purposes. Among other features, we were particularly interested in the technique of rotating a part of the gimbal structure about the indicated vertical axis to reduce friction effects by periodically reversing the direction of undesired torque components with respect to the angular momentum of the gyroscopic rotor. So many detailed considerations became involved as designs were worked out that I can do more than suggest a few of the obvious decisions that started new pa[illegible] of development for inertial systems in 1946. Figure 7, complex as it may appear, is a simplified diagram of an arrangement of elements that was studied on paper and, to some extent, in working hardware directed toward realizing the functions of a stabilized member.

Two, two-degree-of-freedom gyro rotors were mounted within double gimbal systems which in turn were carried on the structure of a stable member that was mounted with pitch, roll, and azimuth freedom in gimbals carried by the airplane framework. Each of the two gimbals for each rotor carried an electrical torque motor. In combination, the motors were arranged so that their outputs could precess the spin axes to any desired orientations with respect to selected reference directions. The gimbals carrying the stable unit were equipped with torque motors having outputs sufficient to overcome friction and inertia effects associated with the support bearings. Each of the torque motors was part of an assembly including a "pick-off" to generate electrical signals representing angular deviations about the various axes. The pick-offs and torque motors were combined in servo-drive loops with suitable electronic power amplifiers so that signals from the gyro units could accurately determine the orientation of the stable unit without imposing disturbing

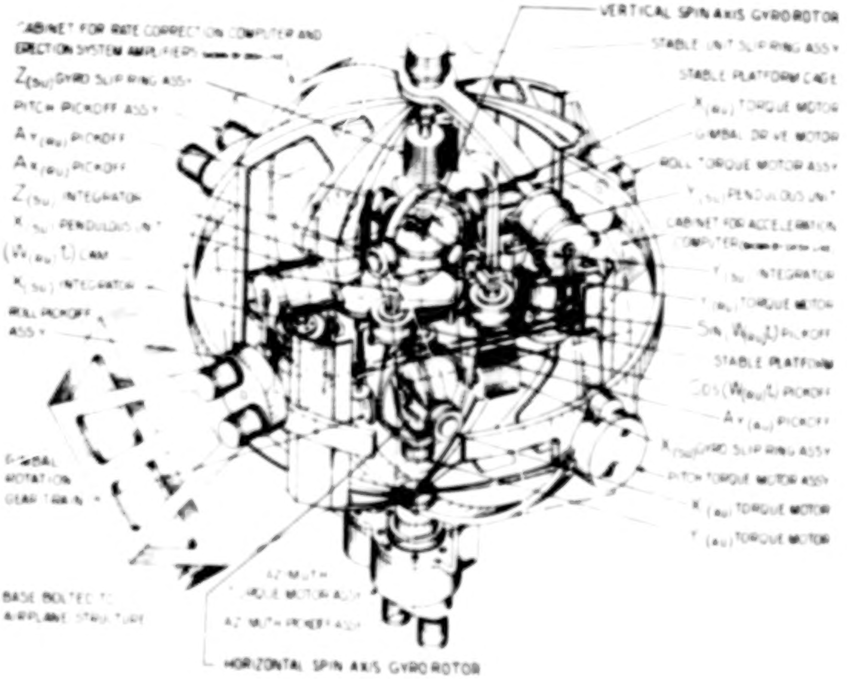

CABINET FOR RATE CORRECTION COMPUTER AND ERECTION SYSTEM AMPLIFIERS
Z(su) GYRO SLIP RING ASSY
PITCH PICKOFF ASSY
A Y(au) PICKOFF
A X(au) PICKOFF
Z(su) INTEGRATOR
X(su) PENDULOUS UNIT
(W(au) t) CAM
X(su) INTEGRATOR
ROLL PICKOFF ASSY
GIMBAL ROTATION GEAR TRAIN
BASE BOLTED TO AIRPLANE STRUCTURE
AZIMUTH TORQUE MOTOR ASSY
AZIMUTH PICKOFF ASSY
HORIZONTAL SPIN AXIS GYRO ROTOR
VERTICAL SPIN AXIS GYRO ROTOR
STABLE UNIT SLIP RING ASSY
STABLE PLATFORM CAGE
X(au) TORQUE MOTOR
GIMBAL DRIVE MOTOR
ROLL TORQUE MOTOR ASSY
Y(su) PENDULOUS UNIT
CABINET FOR ACCELERATION COMPUTER
Y(su) INTEGRATOR
Y(au) TORQUE MOTOR
Sin (W(au) t) PICKOFF
STABLE PLATFORM
Cos (W(au) t) PICKOFF
A Y(au) PICKOFF
X(su) GYRO SLIP RING ASSY
PITCH TORQUE MOTOR ASSY
X(au) TORQUE MOTOR
Y(au) TORQUE MOTOR

Fig. 7
Two-Degree-of-Freedom Gyro Stabilization Unit

torques on the gyro rotors. This application of servo-mechanism technology--to control orientation of relatively heavy members, with reference directions established by gyro unit gimbals without imposing any significant torque on the rotor has been universally adopted by designers of inertial guidance equipment.

Two single-degree-of-freedom pendulum units provided the specific force receiving functions for the arrangement of Figure 7, with torquing and signal generating functions performed by components within the units supporting the axes of the two pendulums. Components to integrate the gyro torque motor input currents and determine the changes in spin axis orientation appear as integral parts of the system. Computing functions are suggested as system parts in Figure 7 by a dashed line indication of a cabinet. The computations to be performed could have been carried out by using a variety of detailed arrangements, but vehicle location, velocity, and motion corrections both as indications and also in terms of control commands were results required from any system.

We found that well-qualified mathematicians could design adequate computers which could be implemented by the technology available. But the instrumentation for realizing complete and accurate geometrical information with on-board equipment suitable for which solutions had to be found. For these reasons a year's work on inertial guidance reaffirmed our earlier decision to concentrate on inertial sensors and the technology of generating and applying geometrical information by self-contained onboard systems.

We chose a reference member stabilized and constrained to follow the direction of the local vertical as the carrying aircraft moved over the Earth's surface. This member provided a basic input from which self-contained equipment could derive navigational indications. Of course, it would be necessary to compensate for the effects of acceleration on the outputs from the sensors in order to achieve accurate indications of the vertical. Directions on the Earth's surface (the heading from North for example) would also be needed. Finally, some geometrical reference for measuring with good accuracy the angular motion of the local vertical was needed to determine distances covered on the Earth's surface. Inertial space could be used as this reference by relating the integrals of torquing inputs applied to the gyro rotors to angles of spin axis rotation by calibrations of the sensors. Another approach involved including a physical member that would hold a selected and established reference orientation with respect to inertial space as part of the overall system. Changes in local vertical directions corresponding to travel over the Earth could be read out in terms of angular displacements with respect to this reference member. By 1951 both of these approaches to the orientational reference problem had been implemented in designs of test models built in the Instrumentation Laboratory.

STABLE VERTICAL BASED ON THREE SINGLE-DEGREE-OF-FREEDOM GYRO UNITS AND TWO SINGLE-DEGREE-OF-FREEDOM PENDULUM UNITS

Reviews of past experiences with aircraft instruments, gunsights, marine equipments, and experiments with specially designed test devices, showed that friction levels obtainable from ball bearings could not be reduced below the order of ten dyne centimeters. Gyro rotors of reasonable weight, a few hundred grams, spinning with angular velocities of several tens of thousands of revolutions per minute provide angular momentum with magnitudes not far from one million gram centimeters squared per second. The corresponding precessional rate for a ten dyne centimeter disturbing torque is 10^{-5} radians per second. Earth's rate is fifteen degrees per hour which is about 7.3×10^{-5} radians per second. It thus appeared that there was no hope of using ball bearing supported gimbals to achieve drift rates that were less than several times Earth's rate with gyro rotors of reasonable size. At fifteen degrees per hour, where each degree corresponds to sixty miles of distance on the Earth's surface, it would be impossible to achieve stability in geometrical reference member orientations permitting navigational uncertainties less than several miles. This led us to conclude that gyro gimbal supports for practical inertial navigation systems could not be realized with ball bearings.

Many possibilities, elastic members after the pattern used for the gunsights, electromagnetic fields, electrostatic fields, hydrostatic pressurized bearings, grease bearings, "squeeze" film bearings, and various other schemes were tried and abandoned for one reason or another. After a number of researches to determine the integrational accuracy that could be achieved by shear drag forces generated from velocity gradients in thin layers of viscous fluids, we decided to use floatation for providing most of the support for a single gimbal structure designed as a hollow float to directly carry gyro rotors and their spin axis mechanisms for single-degree-of-freedom configurations. By adjustment and continuous control of temperatures, floatation forces could account for support of all but a small fraction of one percent of the gimbal weight. As a useful by-product of this arrangement, viscous drag integration of gyro output torque about the gimbal axis could lead to signals representing case rotation about the input axis so that the overall gyro unit would provide angular deviation output signals. In practice, the addition of means for magnetic support without disturbing torque could make it possible to absorb residual non-floated weight and realize gyro units with gimbal output axis friction reduced below one thousandth of a dyne centimeter. These considerations for gyro units were found to apply with equal validity to pendulums and sensors of various types to receive specific force. The same design features were found to be effective remedies for various other difficulties involved.

Engineering studies showed that, except for very short transient periods of alignment, reference directions fixed to the structure of the stable member had to be servo-controlled within small angular deviations from reference directions determined by the gyro spin axes. This made it possible to eliminate one gimbal from each gyro rotor suspension in an arrangement with the features illustrated in Figure 7, and to use single axis signal pickoffs for the remaining single axis gimbals. Pickoffs of this kind working about a single axis needed only a very limited range, on the order of arc seconds, of angular motion to accommodate for any deviation that normally operating servo-loops would ever allow to occur in stable member orientation. Realization of this fact determined the essential pattern of sensor design, and made it possible to use the simplest possible engineering features in construction.

Figure 8 is a 1946 artist's representation of a three-degree-of-freedom-servomotor-powered gimbal arrangement supporting a stable platform. This platform carried three single-degree-of-freedom gyro units with their input axes mutually at right angles to sense angular deviations with respect to inertial space about these three directions. Two single-degree-of-freedom units with external pendulums are shown with their pivot directions fixed to the stable platform. Although details of construction are suggested rather than shown here, the general arrangement of functional components proved so satisfactory that we used it directly or with suitable modifications in many inertial guidance systems designed in later years by the Instrumentation Laboratory.

Figure 9 represents the configuration shown in Figure 8 in a line schematic diagram for a complex of components to operate as the basic mechanical subsystem of the stable vertical. The essential operational relationships, including electrical quantities

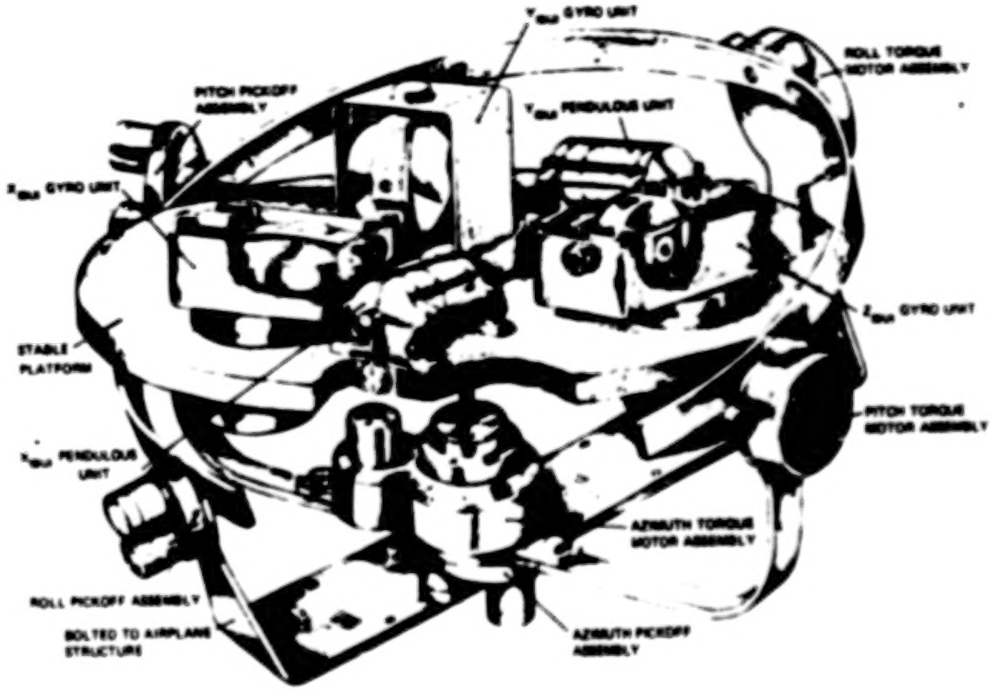

Y_{DU} GYRO UNIT
ROLL TORQUE MOTOR ASSEMBLY
PITCH PICKOFF ASSEMBLY
Y_{DU} PENDULOUS UNIT
X_{DU} GYRO UNIT
Z_{DU} GYRO UNIT
STABLE PLATFORM
PITCH TORQUE MOTOR ASSEMBLY
X_{DU} PENDULOUS UNIT
AZIMUTH TORQUE MOTOR ASSEMBLY
ROLL PICKOFF ASSEMBLY
BOLTED TO AIRPLANE STRUCTURE
AZIMUTH PICKOFF ASSEMBLY

Fig. 8
Single-Degree-of-Freedom Gyro Stable Platform

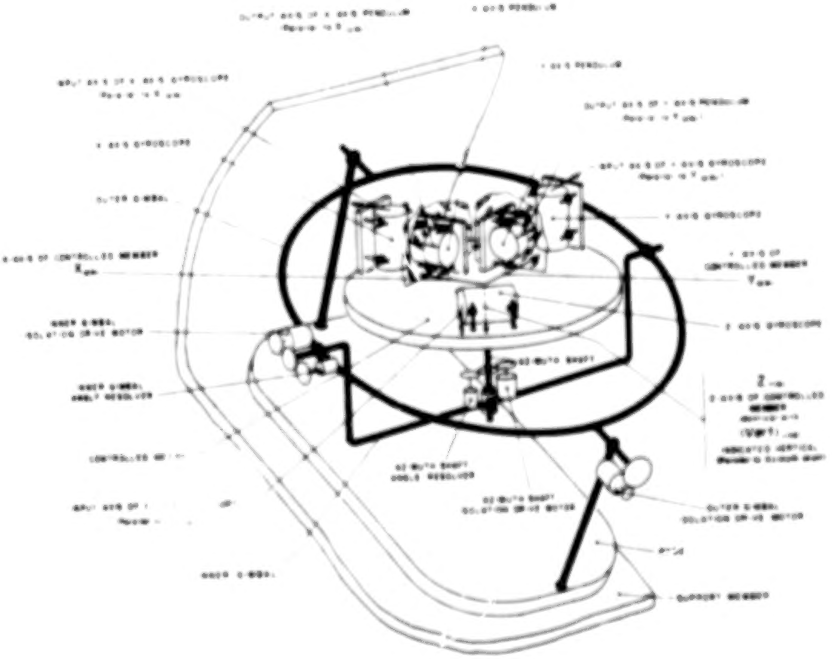

OUTER GIMBAL
X-AXIS OF CONTROLLED MEMBER
X_{CM}
Y-AXIS OF CONTROLLED MEMBER
Y_{CM}
Z-AXIS GYROSCOPE
INNER GIMBAL ISOLATION DRIVE MOTOR
INNER GIMBAL ANGLE RESOLVER
AZIMUTH SHAFT
Z_{CM}
Z-AXIS OF CONTROLLED MEMBER
CONTROLLED MEMBER
AZIMUTH SHAFT ANGLE RESOLVER
AZIMUTH SHAFT ISOLATION DRIVE MOTOR
OUTER GIMBAL ISOLATION DRIVE MOTOR
INNER GIMBAL
SUPPORT MEMBER

Fig. 9
Essential Components of the Stable Vertical

processed by amplifiers and computers, are represented in a functional diagram in Figure 10. A detailed explanation of this diagram would require more words and space than are available here. If an impression of the nature and complexity of the relationships involved is suggested by Figure 10, the writer's objective will have been achieved.

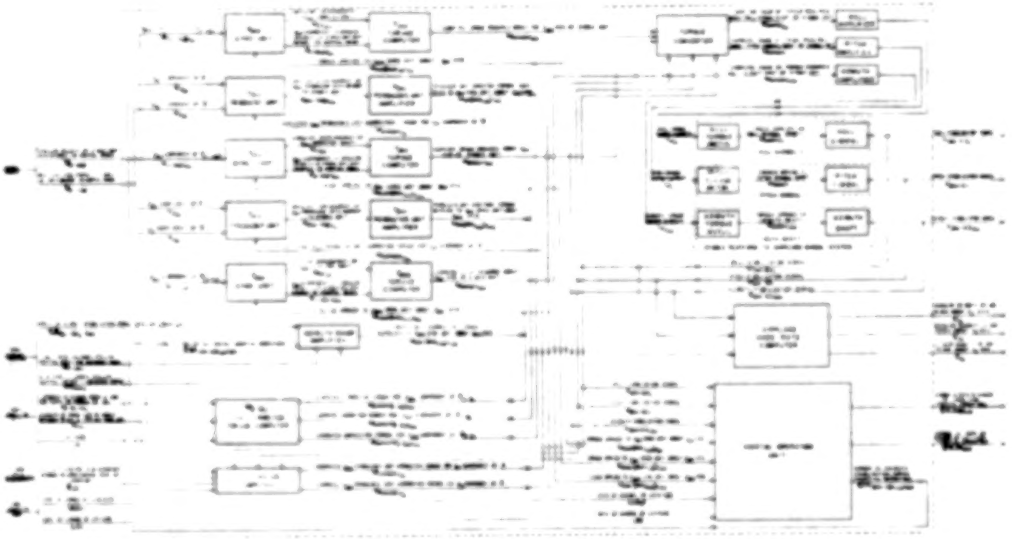


Fig. 10
Functional Block Diagram of Single-Degree-of-Freedom Gyro Stabilization Unit
(Flow of Information Only)

PRACTICAL SINGLE-DEGREE-OF-FREEDOM INERTIAL SENSORS

Single-degree-of-freedom sensors for angular deviations and gravity have been shown in the mechanical subsystem diagrams of Figures 8 and 9. In Figure 8 both units and pendulums are represented as having journal bearings without any remarkable features. Figure 11 is an artist's rendition of the arrangement used in the first experimental pendulum unit. Ball bearings fitted in the instrument case were used at each end of the shaft which carried the pendulum. Salient pole stators mated to direct drive rotors fixed to the shaft, served the functions of generating output signals and applying command torques to the pendulum. An aluminum cylindrical cup mounted on the shaft and rotating in the field of a permanent magnet provided damping for pendulum rotation.

Tests of engineering models incorporating the features shown in Figure 11, and including ball bearing supports for the output shaft, gave angular position uncertainties much larger than the fractional arc minute required for inertial guidance specific force receivers. Studies of all the principles that might overcome this and other difficulties

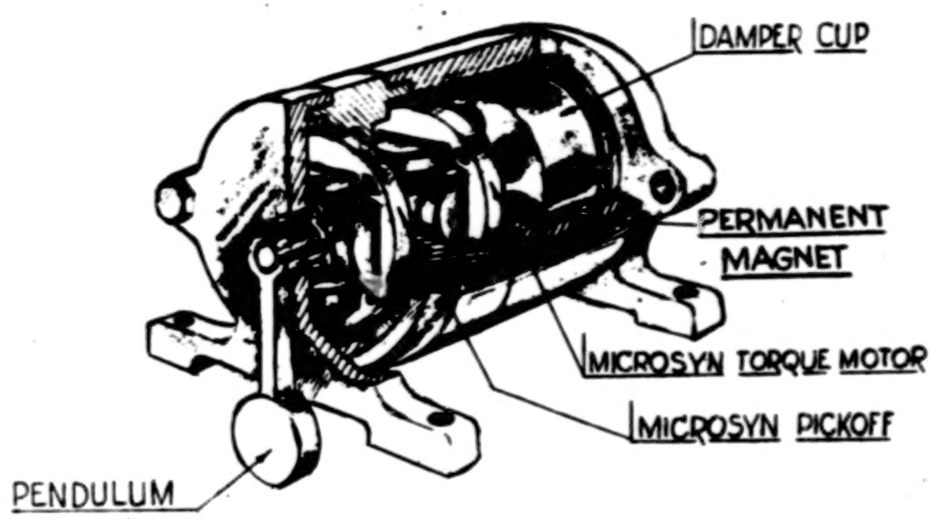

DAMPER CUP
PERMANENT MAGNET
MICROSYN TORQUE MOTOR
MICROSYN PICKOFF
PENDULUM

Fig. 11
Pendulous Unit

led to the choice of floatation support supplemented by polished watch-type pivots and jewels. The general principles applied in the configuration chosen for pendulum unit design are illustrated in Figure 12. The pendulous element was enclosed in a sealed cylindrical chamber weight adjusted to just float in the viscous fluid filling the clearance volume between the cylinder and the case. This depended upon maintaining temperature at the proper level. Figure 13 is a sectioned drawing showing the details of construction for an actual single-degree-of-freedom pendulum unit. Units of this kind were used in the systems that are described later in this paper.

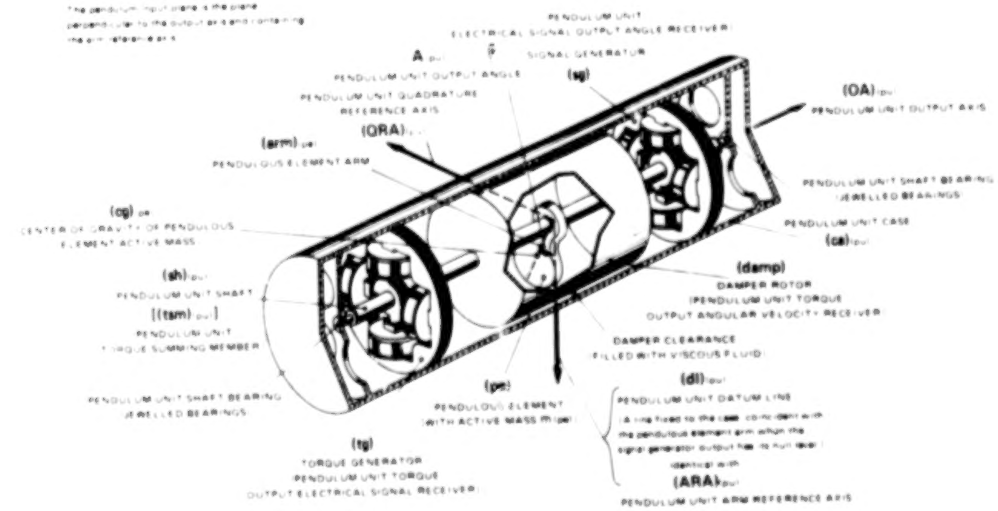

The pendulum input plane is the plane perpendicular to the output axis and containing the arm reference axis
PENDULUM UNIT
ELECTRICAL SIGNAL OUTPUT ANGLE RECEIVER)
A
SIGNAL GENERATOR
PENDULUM UNIT OUTPUT ANGLE
(sg)
PENDULUM UNIT QUADRATURE REFERENCE AXIS
(QRA)
(OA)
PENDULUM UNIT OUTPUT AXIS
(arm)
PENDULOUS ELEMENT ARM
PENDULUM UNIT SHAFT BEARING (JEWELLED BEARINGS)
(cg)
CENTER OF GRAVITY OF PENDULOUS ELEMENT ACTIVE MASS
PENDULUM UNIT CASE
(ca)
(sh)
PENDULUM UNIT SHAFT
[(tsm)]
PENDULUM UNIT TORQUE SUMMING MEMBER
(damp)
DAMPER ROTOR
(PENDULUM UNIT TORQUE OUTPUT ANGULAR VELOCITY RECEIVER)
DAMPER CLEARANCE
(FILLED WITH VISCOUS FLUID)
PENDULUM UNIT SHAFT BEARING (JEWELLED BEARINGS)
(pe)
PENDULOUS ELEMENT
(WITH ACTIVE MASS m (pe))
(dl)
PENDULUM UNIT DATUM LINE
(A line fixed to the case, coincident with the pendulous element arm when the signal generator output has its null level)
identical with
(ARA)
PENDULUM UNIT ARM REFERENCE AXIS
(tg)
TORQUE GENERATOR
(PENDULUM UNIT TORQUE OUTPUT ELECTRICAL SIGNAL RECEIVER)

Fig. 12
Pictorial Schematic Diagram of Damped Single-Degree-of-Freedom Pendulum Unit

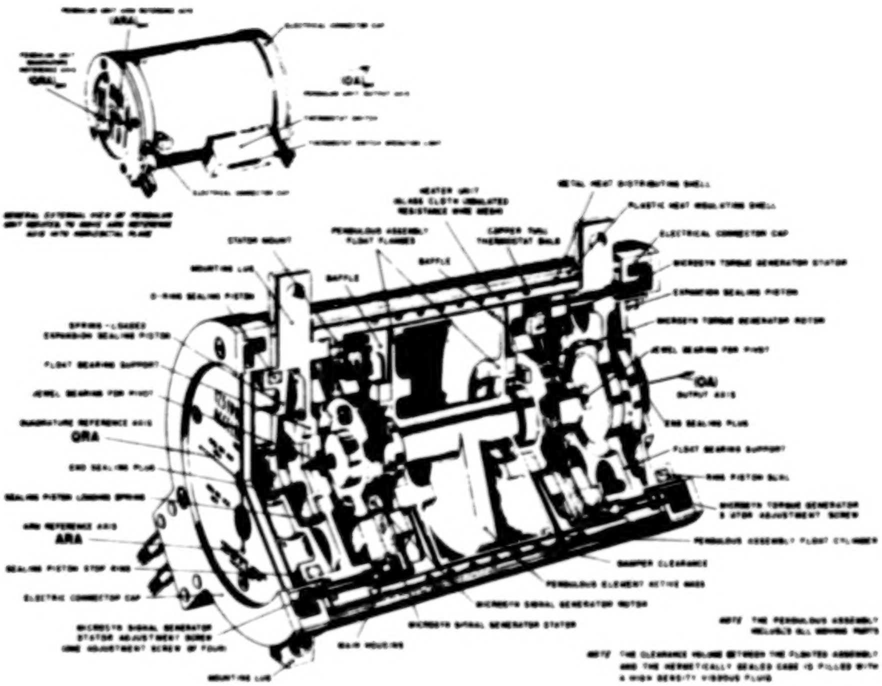

IOA
QRA
ARA
OA
OUTPUT AXIS
ELECTRICAL CONNECTOR CAP
STATOR MOUNT
MOUNTING LUG
BAFFLE
O-RING SEALING PISTON
SPRING-LOADED EXPANSION SEALING PISTON
FLOAT BEARING SUPPORT
JEWEL BEARING FOR PIVOT
QUADRATURE REFERENCE AXIS
END SEALING PLUG
SEALING PISTON LOADING SPRING
SEALING PISTON STOP RING
ELECTRIC CONNECTOR CAP
MAIN HOUSING
HEATER UNIT
COPPER TUBE THERMOSTAT BULB
METAL HEAT DISTRIBUTING SHELL
PLASTIC HEAT INSULATING SHELL
MICROSYN TORQUE GENERATOR STATOR
EXPANSION SEALING PISTON
MICROSYN TORQUE GENERATOR ROTOR
RING PISTON SEAL
PENDULOUS ASSEMBLY FLOAT CYLINDER
DAMPER CLEARANCE
PENDULOUS ELEMENT ACTIVE MASS
MICROSYN SIGNAL GENERATOR ROTOR
MICROSYN SIGNAL GENERATOR STATOR

Fig. 13
Sectioned View Showing Constructional Details of
Single-Degree-of-Freedom Pendulum Unit

Gyro units built with ball bearing gimbals gave entirely inadequate engineering test results. Inaccuracies could definitely be traced to erratic and excessively high minimum torque levels and to susceptibility to torque disturbances under vibration and mechanical shock. Improvements great enough to achieve satisfactory gyro units were made by applying the same principles of floatation and viscous shear damping used in single-degree-of-freedom pendulum units with the features represented in Figure 12. Figure 14 is a corresponding diagram for the floating single-degree-of-freedom gyro unit. The gimbal carrying the spin axis bearings (which were ball bearings in all early units) was sealed in a cylinder from which shafts projected on both ends. The cylinder was adjusted in size and weight to float when the clearance between the cylinder and the case was filled with fluid maintained at operating temperature by means not shown in the diagram. Nuts accessible from outside the case were attached to the float by screws. Adjustment of these nuts in and out along the screws made it possible to realize a very close balance of the moving assembly. This assembly was free to rotate about polished pivots working within watch-type jewels mounted in the case. With almost-perfect floatation accompanying good thermal control, the small loads carried by the jewels reduced uncertainty torque effects sufficiently to achieve performance high enough to demonstrate the feasibility of test systems.

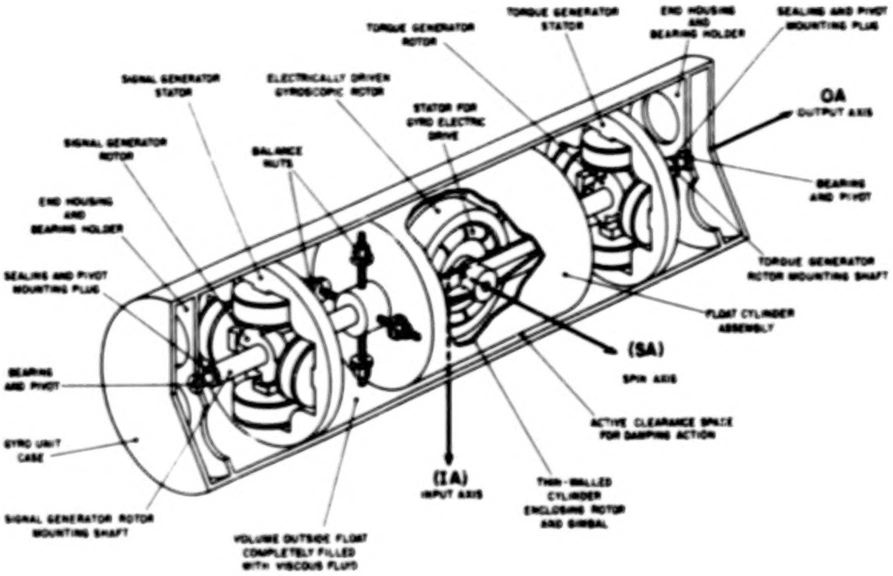

TORQUE GENERATOR ROTOR
TORQUE GENERATOR STATOR
END HOUSING AND BEARING HOLDER
SEALING AND PIVOT MOUNTING PLUG
SIGNAL GENERATOR STATOR
ELECTRICALLY DRIVEN GYROSCOPIC ROTOR
STATOR FOR GYRO ELECTRIC DRIVE
OA
OUTPUT AXIS
SIGNAL GENERATOR ROTOR
BALANCE NUTS
BEARING AND PIVOT
END HOUSING AND BEARING HOLDER
TORQUE GENERATOR ROTOR MOUNTING SHAFT
SEALING AND PIVOT MOUNTING PLUG
FLOAT CYLINDER ASSEMBLY
(SA)
SPIN AXIS
BEARING AND PIVOT
ACTIVE CLEARANCE SPACE FOR DAMPING ACTION
GYRO UNIT CASE
(IA)
INPUT AXIS
THIN-WALLED CYLINDER ENCLOSING ROTOR AND GIMBAL
SIGNAL GENERATOR ROTOR MOUNTING SHAFT
VOLUME OUTSIDE FLOAT COMPLETELY FILLED WITH VISCOUS FLUID

Fig. 14
Pictorial Schematic Diagram of Single-Degree-of-Freedom Integrating Gyro Unit

Alternating current electrical power for the gyro rotor was supplied to the float by flexible leads not shown in Figure 14. This figure shows a signal generator providing signal output from the unit representing the angular deviation about the output axis of the spin axis from its reference position (the direction, fixed in the case, along which the spin axis is aligned when the signal output has its null level). It also shows a torque generator that acted about the gyro output axis to cause the spin axis deviations from the reference alignment. A gyro unit output signal became the input causing the associated servomotor and stable unit structure (when the unit is mounted on a working platform) to rotate. This rotation had the direction of the gyro unit input axis to drive the spin axis toward its null signal orientation. Thus, command signals supplied to the torque motor of a gyro in the complete system caused the stable unit to rotate at a rate and in the direction determined by these signals.

Figure 15, which corresponds to Figure 13 for the pendulum unit, shows a cutaway of the design details of an actual single-degree-of-freedom gyro unit as this sensor was applied in the first inertial guidance systems designed and built at the Instrumentation Laboratory.

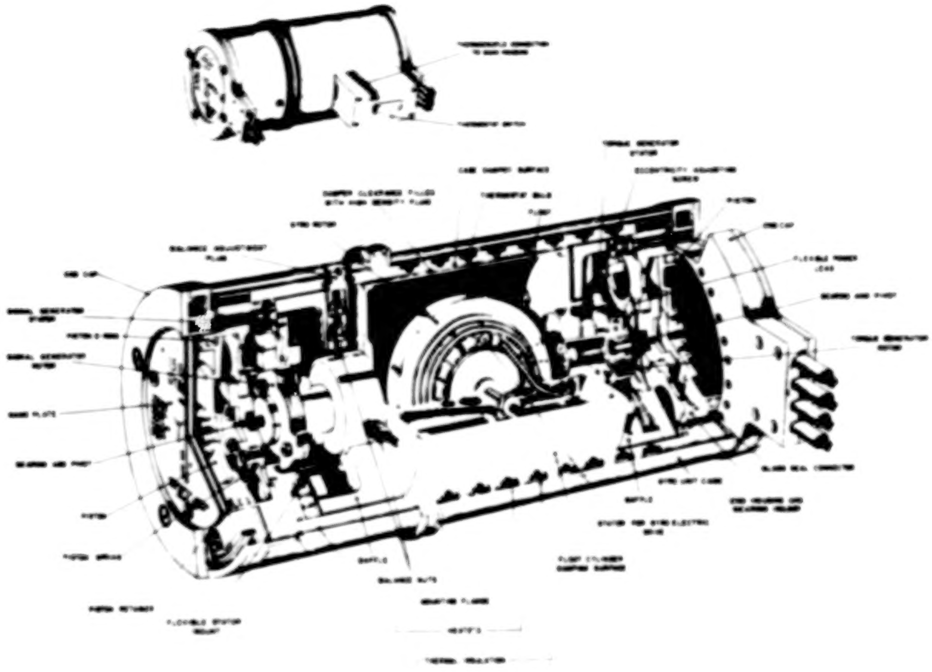


Fig. 15
Cutaway View of 1.5 F.G.-2 Integrating Gyro Unit

FIRST SYSTEM USING INERTIAL PRINCIPLES DESIGNED AND BUILT BY THE INSTRUMENTATION LABORATORY: THE FEBE SYSTEM

Starting in 1946, with no background in technology or any applicable flight experience, the Instrumentation Laboratory directed its efforts toward designing, building, and flight testing equipment that would demonstrate the feasibility of inertial guidance systems, and defining the directions that should be followed by engineering efforts. The Air Force assigned a B-29 aircraft to this task, but we lacked special ground equipment or even maps from which more than a few accurately known check points could be taken. Studies of the overall inertial guidance problem led to the choice of a local vertical indicating platform following the arrangements of Figures 9 and 10. This left the problem of azimuth determination open for the purposes of directional control of the aircraft, and also for navigational indications. Magnetic compass readouts were not suitable because of inherent inaccuracies and unavoidable oscillations or filtering time lags. For these reasons we decided to incorporate an automatic tracker for celestial bodies in the first system. Without adequate radio or radar ground equipment to continuously give aircraft position at night, the celestial body chosen for reference purposes was the sun. This permitted daylight operation and the use of photography for determination of position.

The name chosen for the experimental system was thus PHOEBUS, for the Greek God of the Sun, a name that was soon shortened to FEBE as the official name.

From the standpoint of mechanization, a gimbaled servo-driven tracking member carried an automatic optical tracker for following the line of sight to the sun by means of specially designed elements. Angles of the tracking member with respect to its supporting base, which was fixed to the airplane structure, were read out, and the corresponding signals transmitted to the computing system for use in forming control command signals and navigational locations. These indications, displayed to the human pilot, supplied the information necessary for steering the aircraft. Control command signals generated by the computer were also linked with the automatic pilot so that the Stellar-Inertial Guidance System could keep the aircraft on proper course to its target without attention from human members of the crew. Figure 16 is a side view diagram of the FEBE system installed in the rear compartment of a B-29 aircraft. Figure 17 is the corresponding diagram for the experimental equipment as seen from above. The various components and subsystems are labeled with the names defined in preceding figures of this paper. Figure 18 is a photograph from forward and above within the airplane, showing the actual FEBE installation. The complete system involved a bulk of some 4,000 pounds on the weight and balance chart for the airplane.

A number of test flights were made between Hanscom Field near Boston, Massachusetts, and various airfields in the midwest. Performance involving an error build-up of about ten miles for five hours flying time was achieved on a number of trips from east to west. Results of this kind certainly did not prove that our objectives for

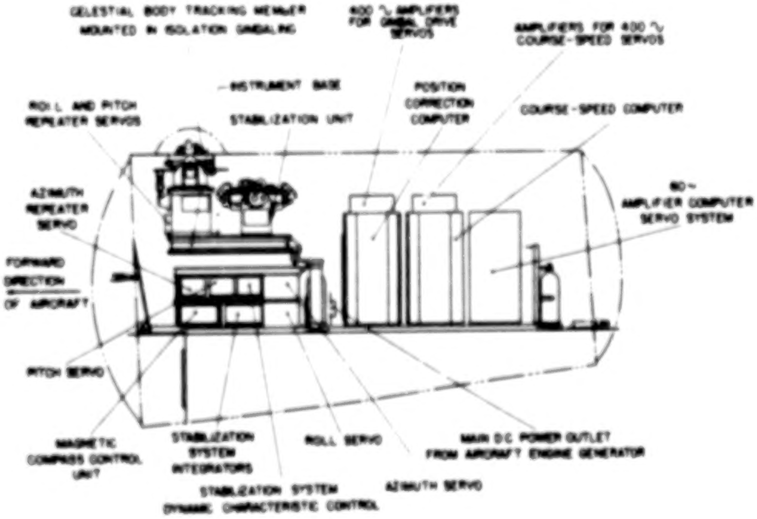

CELESTIAL BODY TRACKING MEMBER MOUNTED IN ISOLATION GIMBALING
400 ∿ AMPLIFIERS FOR GIMBAL DRIVE SERVOS
AMPLIFIERS FOR 400 ∿ COURSE-SPEED SERVOS
INSTRUMENT BASE
POSITION CORRECTION COMPUTER
ROLL AND PITCH REPEATER SERVOS
STABILIZATION UNIT
COURSE-SPEED COMPUTER
AZIMUTH REPEATER SERVO
80 ~ AMPLIFIER COMPUTER SERVO SYSTEM
FORWARD DIRECTION OF AIRCRAFT
PITCH SERVO
MAGNETIC COMPASS CONTROL UNIT
STABILIZATION SYSTEM INTEGRATORS
ROLL SERVO
MAIN DC POWER OUTLET FROM AIRCRAFT ENGINE GENERATOR
STABILIZATION SYSTEM DYNAMIC CHARACTERISTIC CONTROL
AZIMUTH SERVO

Fig. 16
Installation of FEBE System in B-29 Aft Pressurized Compartment Elevation View

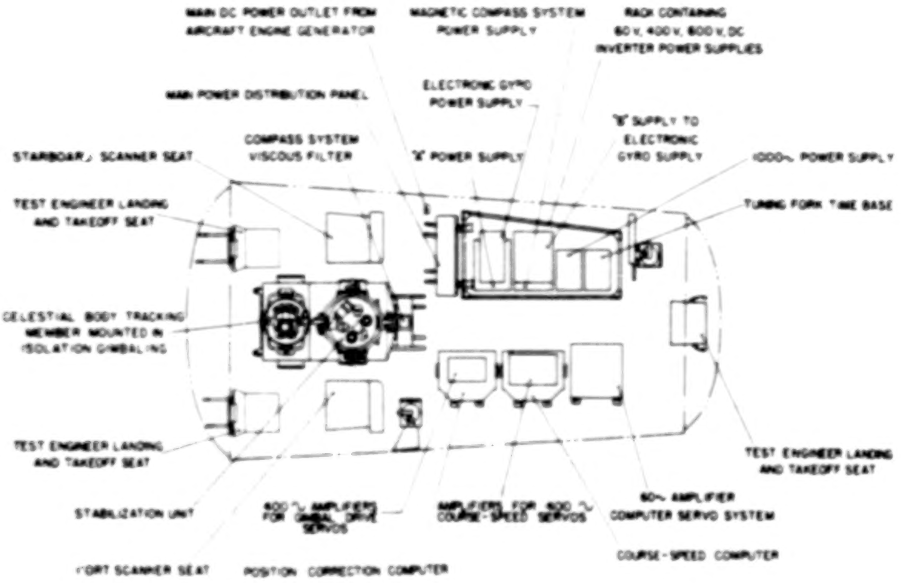

MAIN DC POWER OUTLET FROM AIRCRAFT ENGINE GENERATOR
MAGNETIC COMPASS SYSTEM POWER SUPPLY
RACK CONTAINING 80 V, 400 V, 800 V, DC INVERTER POWER SUPPLIES
MAIN POWER DISTRIBUTION PANEL
ELECTRONIC GYRO POWER SUPPLY
"B" SUPPLY TO ELECTRONIC GYRO SUPPLY
COMPASS SYSTEM VISCOUS FILTER
STARBOARD SCANNER SEAT
"X" POWER SUPPLY
1000~ POWER SUPPLY
TEST ENGINEER LANDING AND TAKEOFF SEAT
TUNING FORK TIME BASE
CELESTIAL BODY TRACKING MEMBER MOUNTED IN ISOLATION GIMBALING
TEST ENGINEER LANDING AND TAKEOFF SEAT
TEST ENGINEER LANDING AND TAKEOFF SEAT
STABILIZATION UNIT
400 ~ AMPLIFIERS FOR GIMBAL DRIVE SERVOS
AMPLIFIERS FOR 400 ~ COURSE-SPEED SERVOS
60~ AMPLIFIER COMPUTER SERVO SYSTEM
COURSE-SPEED COMPUTER
PORT SCANNER SEAT
POSITION CORRECTION COMPUTER

Fig. 17
Installation of FEBE System in B-29 Aft Pressurized Compartment Plan View

the project were attained, but they did give strong indication that guidance systems using inertial-only principles could be designed and reduced to practical operation.

By the end of 1949 it appeared that substantially all of the useful information from the FEBE system had been obtained, so engineering tests were stopped and work began on a second approximation of inertial guidance equipment, this time entirely free from dependence upon tracking of any celestial body.

THE SPIRE SYSTEM

Gyro unit and servodriven stable member behavior had been generally encouraging in the FEBE system, with drift rates in the fractional meru (by meru is one one thousandth of the Earth's rate, i.e., $10^{-3} \times 15°/\text{hour}$ which is approximately one minute of arc per hour) range achieved on numerous occasions. Several features of the experimental sensors could be improved by reasonable engineering changes, and there was strong optimism that all-inertial guidance systems could be built with smaller size, less weight, and higher performance than the FEBE system.

After preliminary studies were completed by the Instrumentation Laboratory, the Air Force sponsored a new inertial-only system, called SPIRE (Space Inertial Reference Equipment). The performance goal for SPIRE was a one or two-mile error build-up during ten hours of flying time. A geometrical reference member carrying three single-degree-of-freedom gyro units with input axes mutually at right angles, supported by servo-driven

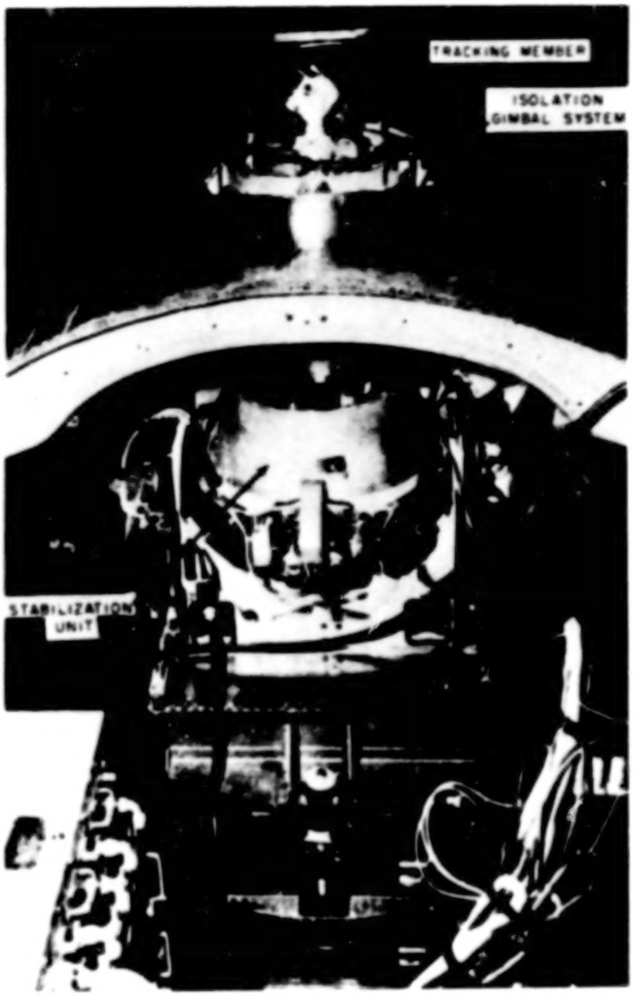


Fig. 18
FEBE System - Installation Showing Stabilization Unit
and Celestial Body Tracking Member

gimbals, allowed SPIRE to maintain a set orientation with respect to inertial space. The orientation chosen for the inertial reference package placed its polar axis along the axis of rotation of the earth. Two gyro units with their input axes at right angles to this polar axis maintained this direction with respect to inertial space. A third gyro unit with its input axis along the polar axis maintained the gyro package non-rotating with respect to inertial space. A drive, powered by an accurately-controlled-frequency source, was designed in to work about the polar axis between the gyro package and next outer supporting gimbal. Rotating this drive at exactly the Earth's rate caused the so-called line of nodes gimbal to remain alligned with the Earth as it rotated in space. The arrangement of the inertial package and its four supporting gimbals is shown in Figure 19.

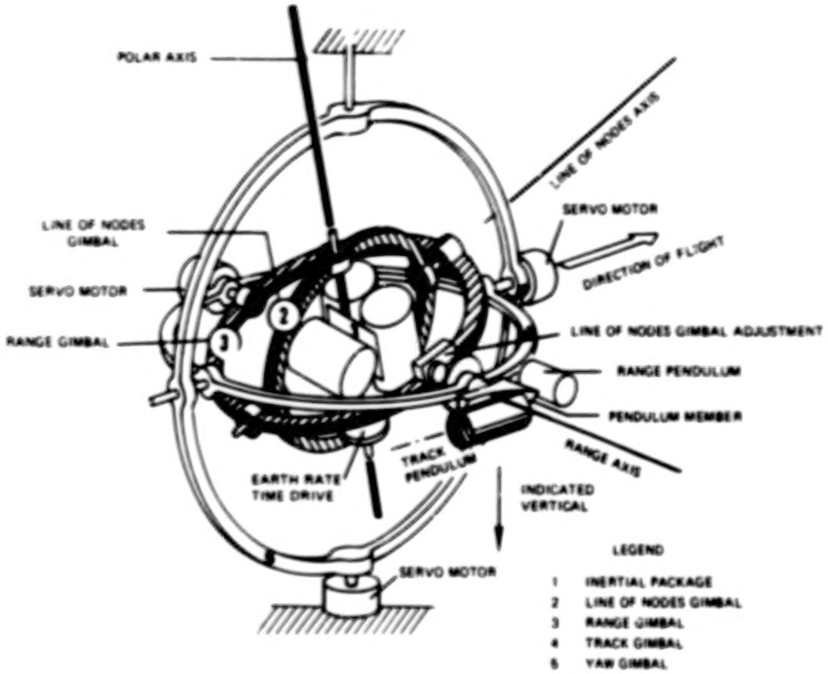

POLAR AXIS
LINE OF NODES AXIS
SERVO MOTOR
DIRECTION OF FLIGHT
LINE OF NODES GIMBAL
SERVO MOTOR
RANGE GIMBAL
2
3
1
LINE OF NODES GIMBAL ADJUSTMENT
RANGE PENDULUM
PENDULUM MEMBER
RANGE AXIS
TRACK PENDULUM
EARTH RATE TIME DRIVE
INDICATED VERTICAL
SERVO MOTOR
LEGEND
1 INERTIAL PACKAGE
2 LINE OF NODES GIMBAL
3 RANGE GIMBAL
4 TRACK GIMBAL
5 YAW GIMBAL

Fig. 19
Track Control

The operating principles of SPIRE allowed us to (1) use the inertial package and Earth rotation time drive to maintain the line of nodes gimbal in alignment with some arbitrary meridian on the Earth's surface, (2) apply two pendulums to sense the local vertical direction in the plane of a preselected great circle course, and (3) measure the rotation of the vertical in this plane to indicate progress along the course. By coupling the pendulum outputs to the aircraft control system with arrangements for automatically nulling the cross track pendulum to keep the aircraft in the plane of the desired great circle course, and comparing the processed output of the range pendulum with the programmed flight position of the aircraft to control location along the track, the SPIRE system could be made to follow a preset flight plan with indications of cross track deviations along the course, and signal mission completion when the objective was reached.

During preflight alignment, the angle between the line of nodes gimbal and the range gimbal was set and clamped so that the proper angle existed from the polar axis to the range axis. For initial adjustment purposes only, the correct range angle was set between the vertical package (unit carrying the two pendulums), and the range gimbal. The stabilization servo-drives were then energized, and the signals from the two pendulums and an externally mounted photo theodolite were resolved and used to torque the gyros thereby aligning the vertical package to the local direction of gravity and turning the range axis to the correct azimuth. With the system geometry established, SPIRE was ready to supply control commands to the autopilot for automatic operation, and indications to readout dials for monitoring by human operators.

Figure 20 is a photograph of the SPIRE system as it appeared during engineering tests in the Laboratory during the latter part of 1951. The system was much larger than could be accepted for aircraft, and weighed about 2800 pounds. Although weighing some 25 percent less than FEBE, it was still much too great for flight use. However, the objective of all-inertial operation had been attacked, and patterns of design for components and subsystems suitable for aircraft purposes were emerging that held real promise for practical guidance equipment with all the qualities needed for flight operations.

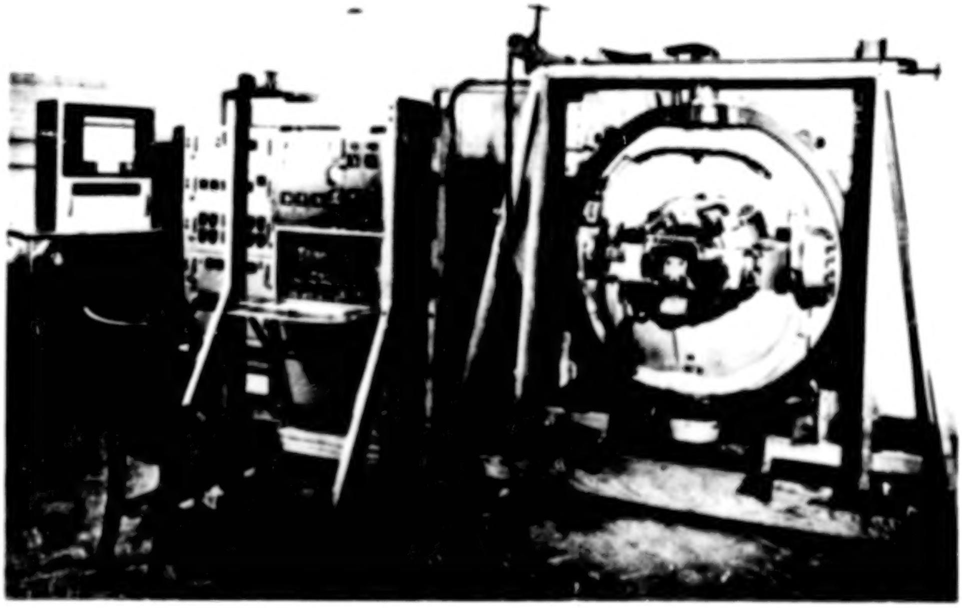


Fig. 20
Photograph of Spire System on Test in Laboratory

CONCLUSION

The "20-Year Rule" set by the Academy History Committee limited this memoir to the 1935-1951 period at the Massachusetts Institute of Technology. During this span of years the basic ideas of inertial guidance appeared, were implemented in experimental hardware, and flight tested to demonstrated engineering feasibility. It is unfortunate in a way that in 1951 the SPIRE flights had not yet started and marine, missile, space, and commercial aircraft developments were still a few years in the future. I will not mention the equipments built and results achieved during the 1950s beyond noting that the technology pioneered, would be followed by considerable production and wide-spread use, based on concepts that were no more than misty ideas in 1935.

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EARTH SATELLITES, A FIRST LOOK BY THE UNITED STATES NAVY[+]

R. Cargill Hall (USA)[++]

ORIGINS

Early in 1945 a special United States Naval Technical Mission in Europe accompanied Allied forces advancing through the low countries into Germany. Concerned with Axis technical and scientific matters, organized into Combined Advanced Field Teams composed of both British and American scientists, the mission members included the mathematician Anthony Biot, aerodynamicists Clark Millikan and Hsue-shen Tsien, and physicists Abraham Hyatt and George Ewald. These men conducted on-the-spot assessments of designated German scientific groups and installations of interest to Allied agencies.

On May 5, 1945, a few of the mission members interrogated the leaders of the Dornberger-von Braun rocket group who had surrendered to Allied troops a few days earlier at Reutte in the Tyrol. During the meeting in the small town of Kochel, Bavaria, Tsien[1] requested that von Braun prepare a summary of German rocket developments and his predictions for the future of rocketry and astronautics.[2] The resulting report, Survey of Development of Liquid Rockets in Germany and Their Future Prospects, suggested potential applications for earth satellites, manned space stations, and flights to neighboring celestial bodies including the moon, as suggested earlier by Tsiolkovsky, Goddard, Oberth, Noordung, and others.[3] In July, that report accompanied Abe Hyatt when he returned to the Navy Bureau of Aeronautics (BuAer) in Washington, D.C. According to Hyatt, it stirred a good deal of interest among individuals at BuAer, especially among those in the Aviation Design Research Branch directed by Ivan H. Driggs.[4] But the individual first and certainly most moved by the subject of earth satellites at BuAer was a young Navy lieutenant, Robert P. Haviland.

[+] Presented at the Foruth History Symposium of the International Academy of Astronautics, Constance, German Federal Republic, October 1970.

[++] Jet Propulsion Laboratory, California Institute of Technology.

Haviland, attached to the Electronics Materiel Branch of the Engineering Division in the Navy Bureau of Aeronautics, examined Allied intelligence reports and other documents relating to aircraft identification and electronic countermeasures. With a decrease in work assignments near war's end, Haviland turned his attention to a favorite topic: the implications of space rocketry.[5] In the late summer of 1945 he read a British report concerning the capabilities of the German V-2 rocket, and a NACA technical translation of some earlier work pursued by Eugen Sänger. Willy Ley's Rockets, the Future of Travel Beyond the Stratosphere (New York: Viking Press, 1944), which treated the energy requirements necessary for orbital flight and escape from the earth's gravitational attraction, also stimulated the young man.[6]

Aware of the discussions in the Aviation Design group and, apparently, the von Braun paper, Haviland set about considering potential space missions, and he spent several weeks exploring the technical feasibility of earth satellites based upon extensions of conventional V-2 rocket technology. On August 10, 1945, he combined the results of this study in a nine-page memo to the Head of the BuAer Special Weapons Section, recommending that an earth satellite project, "Project Rex," be authorized by the U.S. Navy. Although overlooking the problems of atmospheric reentry, Haviland calculated that by clustering and staging large rockets, manned artificial earth satellites were technically feasible, and he recommended a number of potential missions: as a platform for scientific research, as communications relay stations, and for use in mapping and meteorological surveillance.[7]

The President's announcement of the existence of the atomic bomb on August 6--which suggested a potential source of large amounts of energy--enhanced Navy interest in Haviland's proposal. It quickly won the support of his immediate superior, Commander J. A. Chambers,[8] and from Captain Lloyd V. Berkner, himself destined to play a significant role in American astronautics. Captain Berkner, Head of the Electronics Materiel Branch, was much taken with the idea of a communications satellite employed as "an artificial ionosphere,"[9] and he convinced his superiors of the desirability of investigating the prospect of earth satellites.[10] With authorization to proceed, Berkner discussed the matter with Commander Harvey Hall,[11] Special Scientific Assistant to the Head of the Radar Section. Hall agreed to assist in a committee examination of the technical feasibility of earth satellites. On October 3, 1945, BuAer organized a Committee for Evaluating the Feasibility of Space Rocketry.

THEORETICAL VERIFICATION OF A SINGLE-STAGE SATELLITE ROCKET

Between mid-August 1945, when the Haviland memo was brought to the attention of Berkner, and the creation of the satellite committee on October 3, Harvey Hall determined that a truly practical and useful earth satellite could not be realized with existing

V-2 propellants and technology; a major advance in rocket technology, he reasoned, appeared necessary and desirable.[12] Assigned at this time to evaluate the application of jet propulsion to aircraft (which are "single-stage" vehicles), he also considered the avenues for advances in rocketry. Hall struck on the concept of a single-stage satellite rocket. With the satellite committee established, he successfully pressed this approach among associates. Accordingly, the committee was charged "to investigate presently available materials and techniques and arrive at some estimate of the possibility of attaining a velocity of libration from one stage of the operation."[13]

On October 29, 1945,[14] the satellite committee concluded that the possibility of placing a single-stage space ship in orbit above the earth warranted establishing a project within BuAer to conduct a detailed study. A high mass-ratio, single-stage vehicle that would compensate for structural development problems by eliminating staging and reignition of engines at upper altitudes, and that burned high specific impulse liquid propellants such as hydrogen and oxygen, the members suggested, would provide a more powerful as well as reliable satellite rocket.

By the end of November 1945, considerable support had been generated within BuAer for an Earth Satellite Vehicle[15] program, and functional-type project desks appeared in various sections as the satellite studies intensified.[16] The satellite's proponents were now more numerous, and influential: Captain D. S. Fahrney, Head of the Special Design Branch which handled BuAer guided missile projects, Captain W. P. Cogswell[17] of the BuAer Radio and Electrical Group, and shortly thereafter, Rear Admiral Leslie Stevens, Assistant Chief for Research and Development, BuAer. Stevens, though he accepted the satellite's usefulness for communications and as a relay station for guidance of surface-to-surface missiles, nonetheless remained very dubious about obtaining the money necessary to build one in view of the congressional cuts in the post-war Navy budget already taking place.[18] But most of the supporters were optimistic. Captain Fahrney created a special desk in the Pilotless Aircraft Division "to handle all the engineering aspects of the Earth Satellite Program," and selected Lt. Commander R. P. Haviland, USNR[19] to manage it.

Plans for this early Navy satellite project (not to be confused with the later Vanguard Program) called for three sequential phases leading to eventual flight testing: (1) the investigations and preliminary research recommended by the satellite committee, (2) a general engineering study by one or more contractors based upon the satisfactory completion of the preliminary research, and (3) award of a contract for construction and operation of a prototype vehicle.[20] Subsequent Navy work incorporated the satellite committee premise of a single-stage satellite rocket, and proceeded largely on the strength of a hydrogen-oxygen rocket motor development program conducted by the Aerojet Engineering Corporation,[22] supervised by Lt. Commander Robert Truax. Late in 1945, Aerojet personnel succeeded in burning gaseous hydrogen and oxygen in a small rocket motor for the first time, and in the following year they placed the largest hydrogen liquefier in the world in operation at Aerojet; liquid hydrogen was successfully stored, pumped, and burned in what became routine test operations.[23] Within a year the BuAer effort in large rocket propulsion systems involved serious "hardware" development contracts.[+] Additional contracts were modified or begun for the study of boro-hydride fuels and electronic components that could be employed in rockets.[24]

But the Navy Bureau of Aeronautics first sought confirmation of the satellite committee's initial findings in a contract with the Jet Propulsion Laboratory (JPL) at the California Institute of Technology. This contract, executed on December 12, 1945, called for more detailed theoretical research on the relationships between aerodynamic drag during ascent and the desired orbit (altitudes), rocket motor and propellant performance, structural characteristics (mass ratio), payload, and suitable ascent trajectories.[25] JPL undertook these analyses predicated upon the prior satellite committee calculation and reports, and the Navy-stipulated assumptions that:

a. The orbiting missile to be considered is a single-stage liquid propellant rocket missile.

b. The rocket motor propellants are liquid oxygen and liquid hydrogen.

c. The performance of this rocket motor is estimated from experimental data on hydrogen-oxygen propellants obtained by D. A. Young of the Aerojet Engineering Corp.[26]

The study program quickly encountered problems. JPL personnel sought to configure ascent trajectories to achieve a desired circular orbit at an altitude of 240 kilometers (150 miles), a profile assumed to be above substantial interference from atmospheric drag. Both vertical and inclined ascent trajectories, given a specified engine burning period and the assumptions furnished by the Navy, yielded orbital altitudes only up to 80 kilometers (50 miles) where air resistance would not permit even one complete orbit of the earth with lightweight structures. This difficulty, already considered in BuAer, prompted an important innovative contribution to astronautical theory. In the words of JPL's Homer J. Stewart:

> A method for attaining a higher orbiting altitude than it was feasible to attain in a simple powered launching trajectory was proposed by Commander [Harvey] Hall. In this method the missile is projected, at the termination of its primary powered period, into an elliptic orbit of small eccentricity instead of a circular orbit (zero eccentricity). This can be done either by projecting the missile horizontally with a speed in excess of the circular orbital velocity or by projecting the missile at a small angle of climb. In either case, the missile climbs in an elliptic orbit to the aphelion [apogee] point. If a proper velocity increment is added at the aphelion point, the missile is then projected into a circular orbit at this increased altitude. This requires an additional start-stop control for the motor and an altitude [sic, read attitude] control for the missile.[27]

[+] See G. H. Osborn, R. Gordon, and H. L. Coplen, with G. S. James, "Liquid Hydrogen Rocket Engine Development at Aerojet, 1944-1950," in this volume - Ed.

BuAer and JPL-Caltech adopted this ascent technique as a correlative axiom, and used it in all subsequent engineering studies for the high altitude test vehicle (HATV). It has since become standard procedure in all space programs for configurating ascent trajectories, for altering orbital altitudes and eccentricity, and, with later modifications for staged vehicles, for improving payload capacity for a given mass ratio. Unknown to those at work on the Navy satellite studies, the same innovation had been advanced many years before by the Russian astronautical pioneer K. E. Tsiolkovsky.

JPL conducted these studies between December 1945 and July 1946 under the direction of Homer J. Stewart and Frank J. Malina. Using the stipulations and ascent technique suggested by the Navy, JPL calculations[28] confirmed the technical feasibility of a single-stage liquid hydrogen-oxygen satellite rocket attaining a minimum circular earth orbit at 240 km (150 miles) altitude provided: the fuel and engine combination would perform according to theory, and that a projected propellant-gross structure weight (mass) ratio of 0.885 to 0.895:1 could actually be realized. At the same time, in a final report, JPL participants felt obliged to remind the Navy that "the required propellant-gross weight ratio could be greatly reduced if a step rocket [staging] were used. For example, a two-step rocket would require, for each step, a propellant-gross weight ratio of about 0.7."[29]

Encouraged by the Caltech work, on July 1, 1946, the Navy awarded a second contract to Aerojet Engineering. BuAer wanted Aerojet to determine whether a test stand value of the specific impulse of liquid hydrogen-oxygen was sufficiently near the theoretical value to justify the JPL conclusions of satellite feasibility, perform a design study of a liquid hydrogen-oxygen rocket engine producing 136,000 kg (300,000 pounds) thrust, and fabricate a larger hydrogen-oxygen test engine rated at 454 kg (1000 pounds) thrust.[30] These propulsion system contracts led to the construction of the large hydrogen liquefier mentioned previously, and actual test stand operation of the liquid hydrogen-oxygen rocket engines that did confirm theoretical performance specifications.[31] Another contract was awarded to North American Phillips Company to design a lightweight solar engine with a 50 watt electrical output to power satellite electronic equipment, and work commenced in BuAer on the guidance and attitude control equipment required for the period of unpowered coast along an elliptical trajectory prior to insertion in a circular orbit, and for orbit operations.[32] Verification of the structure mass ratio for the single-stage vehicle, the second condition stipulated by JPL, also began in mid-1946 as the Navy moved into the engineering design phase of the earth satellite project.

Meantime, before most of these early investigations had been completed, support for the satellite project was solicited from many individuals and at many levels in the Navy outside of BuAer. It soon became apparent that full Navy support for an actual flight test vehicle program--as Admiral Stevens suspected--would be difficult to achieve. In November 1945, costs for the engineering and preliminary design phase of the project had been estimated at $5 million to $8 million.[33] The postwar demobilization and cutback in Navy appropriations in early 1946 clearly indicated that whatever progress the service might attain would have to be managed on a great deal less than $5 million.[34]

SATELLITE INTEREST IN THE ARMY AIR FORCES

Faced with this unpromising fiscal situation, members of the Navy satellite committee approached the Army Air Forces in the hope of establishing a joint earth satellite project. A joint project, they believed, might be able to command financial support sufficient for flight tests. The first meeting between representatives of the two services took place in Washington, D.C. on March 7, 1946, in the office of the Air Force Major General H. M. McClelland. Navy personnel presented the objectives and status of their rocket satellite project, together with a suggested plan for the proposed Army Air Forces-Navy experimental program to evaluate, justify, and, if warranted, construct and launch a prototype vehicle. Reception of the Navy proposal by the Army Air Forces representatives seemed most favorable; furthermore:

> It was agreed at the conference that the general advantages to be derived from pursuing the satellite development appear to be sufficient to justify a major program, in spite of the fact that the obvious military, or purely naval applications in themselves, may not appear at this time to warrant the expenditure.
>
> On this basis, the Army representatives agreed to investigate the extent of Army interest by discussions with General LeMay and others, after which a future joint conference is planned.[35]

For several days in March 1946 it appeared that a joint satellite project leading to flight tests might possibly commence in the United States.

After carefully reviewing the Navy proposal, General Carl Spaatz, Army Air Forces Chief of Staff, designated Major General Curtis E. LeMay, recently appointed Deputy Chief of Air Staff for Research and Development, to represent the Army Air Forces in negotiations with the Navy satellite proposal team. In mid-March the General informed a dismayed Commander Hall that the Army Air Forces had decided not to support the proposed joint satellite project, although he left open the possibility of further discussions of earth satellites.[36] Army Air Forces representatives confirmed the Spaatz-LeMay decision at a second meeting in the Research and Development Committee of the Aeronautical Board on April 9, 1946. Air Force Brigadier General Laurence Craigie declared two separate programs preferable to one.[37] If earth satellites were to be constructed, Craigie implied, the Army Air Forces intended to be the service to do it, especially since the Army Air Forces had already staked a claim to future military missions in outer space.[38]

Personnel at the Navy Bureau of Aeronautics returned to their single-stage satellite rocket investigations with the groups at JPL and Aerojet. Though the encounter with the Army Air Forces had been a disappointment, it had one positive ramification. It prompted another extensive satellite study. The Army Air Forces Research and Development organization turned to the Project RAND research group and, as a first task, instructed this newly formed consultant organization to perform a separate earth satellite feasibility study.[39] Attempts by the Navy Bureau of Aeronautics to arrange another conference met with repeated delays while the Air Force satellite study progressed.

On May 15 the respective service representatives met again in the Research and Development Committee of the Aeronautical Board. This Board, formed during World War I and composed of ranking members from the Army and Navy air arms,[40] normally met at monthly intervals to review new developments and to reconcile "the viewpoints of the two services for the mutual benefit of aviation."[41] At this third meeting AAF General Craigie introduced the Project RAND satellite study "on which the ink was hardly dry, as the basis of a bargaining position that the AAF was on an equal or similar development position with the Navy," Harvey Hall reflected. "From this point on, no further progress was made towards a joint project."[42] But the meeting, admittedly convened for the purpose of coordinating "the initial phases of a [satellite] project . . . and to define service responsibility, if desirable at this time," bespoke the serious contemporary interest in an artificial earth satellite:

a. It is the most promising means that has been suggested for providing guidance for guided missiles and pilotless aircraft at ranges in excess of about 250 miles.

b. It would provide means for freeing world-wide communications of periodic lack of reliability attributed to outer atmospheric effects.

c. It might be of much value in extending the understanding of meteorology, with consequent improvement in meteorological predictions.

d. It would provide an initial step toward the eventual construction of inhabited satellites and interplanetary travel, with far-reaching implications.[43]

Lacking agreement on which service should be responsible for actual development of prototype satellite vehicles, however, the members referred the question of jurisdiction back to the full Aeronautical Board for a decision. The Navy members maintained steadfastly "that the unknowns involved in this project, and its implications for the possibility of space travel, render premature any attempts to define Service responsibility at this time . . . [but actual development of a satellite should] be undertaken jointly by the Army, the Navy, and civilian science. The Army members consider that because intercontinental warfare is of paramount interest to the Army Air Forces, future plans involving actual construction of an earth satellite should be under the control of that organization . . ."[44] In the face of these service positions, the Research and Development Committee could only recommend that the Army Air Forces and the Navy continue separate preliminary investigations. Meantime, the committee established a formal subcommittee on earth satellites to further examine its potential applications, ensure coordination, and prevent duplication of efforts between the services.[45]

SATELLITE ENGINEERING DESIGNS

Without agreement on joint responsibility, a well-defined use for earth satellites, or project approval from the Aeronautical Board, the Navy satellite study project by itself could not command broad internal support and the necessary funding. Navy participants now realized that construction and launch of a prototype satellite--calculated to cost between $50 and $150 million--was at least temporarily precluded. But further "preliminary investigations" were assured on May 8, 1946, when the Chief of Naval Operations formally approved the BuAer satellite study project,[46] and the second phase of work began: vehicle engineering design. In June and July the Navy Bureau of Aeronautics contracted for two major structural design studies, the first with North American Aviation, and the second with an old Navy stalwart, the Glenn L. Martin Company of Baltimore, Maryland. Both firms teamed with Aerojet Engineering Corporation on the engine and propellants.[47]

On September 26, 1946, North American Aviation submitted its report to BuAer. The design study, directed by William A. Bollay, had been carried out in the firm's newly established Aerophysics Laboratory. R. G. Wilson served as Project Engineer, with individual technical sections prepared by L. A. Gore, responsible for propellants, surface heating, and power plant; Bruno W. Augenstein, aerodynamics; R. G. Wilson, weight, balance, structural and general design; and R. G. Knutson, guidance and control. Each of the contributors based his work on the Navy stipulated single-stage satellite rocket burning liquid hydrogen-oxygen, and the JPL specified mass ratio of 0.895:1 maximum. North American recommended a pressure stabilized ogive structure 26 meters (86 feet) long and with a maximum diameter of 4.9 meters (16 feet). "It is constructed of stainless steel and has nine clustered individual thrust motors. The vehicle is capable of attaining a maximum velocity of 6,820 meters per second (25,400 feet per second) at an altitude of about 225 kilometers (140 miles)"[48] (Figure 1).

North American designed the High Altitude Test Vehicle to weigh 41,130 kg (101,400 lbs), broken down into major components: Propellants, 40,000 kg (89,000 lbs); payload, 450 kg (1,000 lbs); motor and accessories, 2,250 kg (5,000 lbs); and structure, 2,900 kg (6,400 lbs). The starting weight required an initial thrust of 105,700 kg (233,000 lbs) varying to about 139,700 kg (308,000 lbs) thrust at altitude.[49] The firm recommended an eventual longitudinal scaling of the vehicle to about 59,000 kg (130,000 lbs) gross weight with no increase in the vehicle's maximum diameter in a final configuation, and an extension in the burning time of the engines from 126 to 165 seconds.

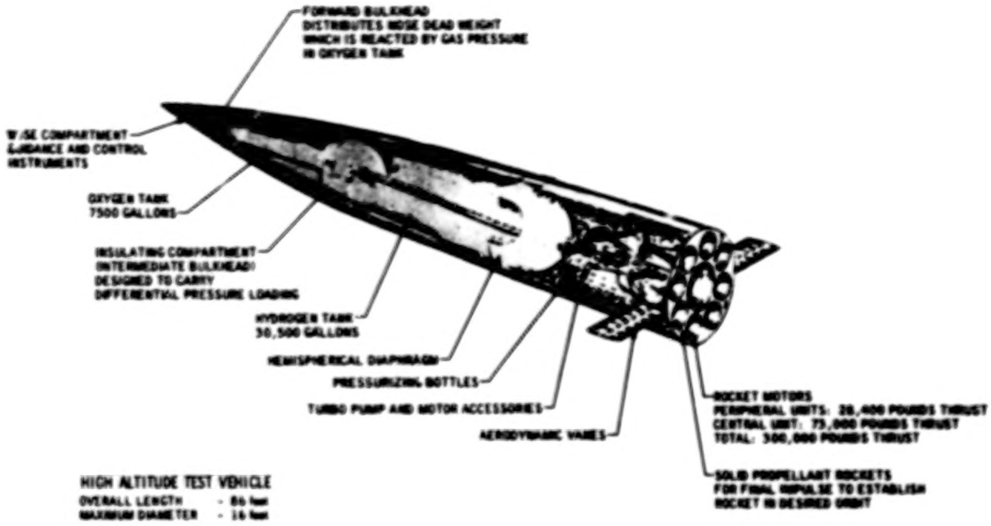
FORWARD BULKHEAD
DISTRIBUTES NOSE DEAD WEIGHT
WHICH IS REACTED BY GAS PRESSURE
IN OXYGEN TANK
NOSE COMPARTMENT
GUIDANCE AND CONTROL
INSTRUMENTS
OXYGEN TANK
7500 GALLONS
INSULATING COMPARTMENT
(INTERMEDIATE BULKHEAD)
DESIGNED TO CARRY
DIFFERENTIAL PRESSURE LOADING
HYDROGEN TANK
30,500 GALLONS
HEMISPHERICAL DIAPHRAGM
PRESSURIZING BOTTLES
TURBO PUMP AND MOTOR ACCESSORIES
AERODYNAMIC VANES
ROCKET MOTORS
PERIPHERAL UNITS: 28,400 POUNDS THRUST
CENTRAL UNIT: 75,000 POUNDS THRUST
TOTAL: 300,000 POUNDS THRUST
SOLID PROPELLANT ROCKETS
FOR FINAL IMPULSE TO ESTABLISH
ROCKET IN DESIRED ORBIT
HIGH ALTITUDE TEST VEHICLE
OVERALL LENGTH - 86 feet
MAXIMUM DIAMETER - 16 feet

Fig. 1

Since the structures portion of this projected single-stage vehicle could not weigh much more than five percent of the total weight of the satellite rocket, the specifications called for a propellant-to-structure (content-to-shell) ratio approaching that of an egg. This task, at once an engineer's aspiration and nightmare, eventually would be realized.[50]

The Glenn L. Martin Company and Aerojet studies continued for a complete fiscal year, ending in June 1947. In its first progress report, Martin proposed the use of a standard honeycomb shell construction if the strength requirements could be maintained within the temperature and pressure ranges encountered during flight into earth orbit.[51] Under subsequent testing, engineers found this material inadequate to meet the necessary regime, and they dropped it in favor of the North American thin-skin, stainless steel, pressure-stabilized "blimp" structural scheme. Martin's extensive final report, which incorporated the Aerojet findings on a 136,000 kg (300,000 lb)-thrust liquid hydrogen-oxygen engine, described a truly remarkable single-stage satellite rocket embodying virtually all of the subsystem features presently found in separable booster rockets and spacecraft. This report was prepared by Pedro C. Medina and William B. Bergen, with support from Charles H. Harry, who handled vehicle temperatures and trajectories, and Albert J. Devaud, responsible for devising the novel ascent guidance and control and orbital attitude control systems[+] (Figure 2).

[+] See R. E. Roberson, "Evolution of Spacecraft Attitude Control Concepts Before 1952," in this volume - Ed.

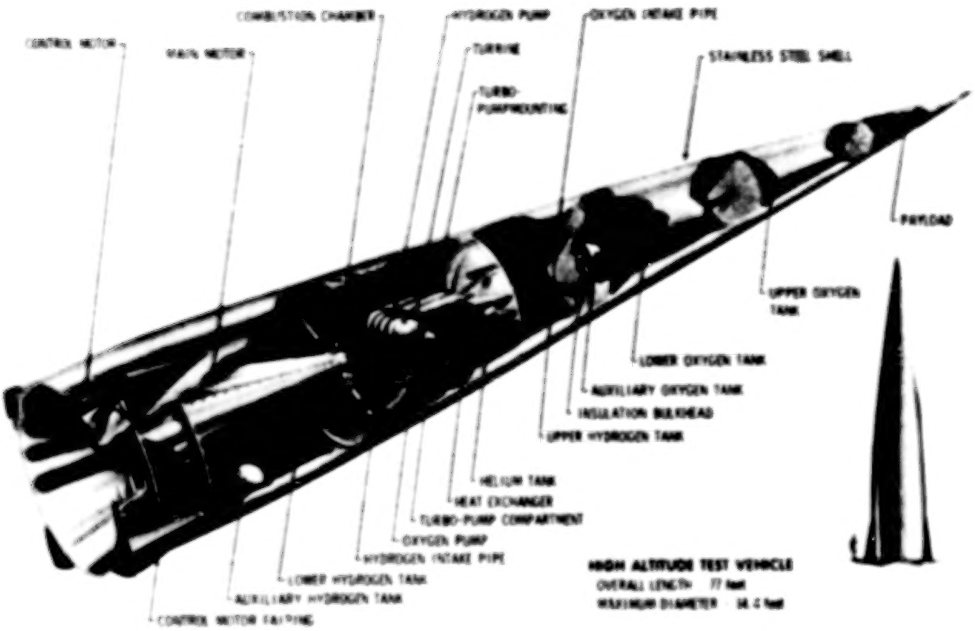
CONTROL MOTOR
MAIN MOTOR
COMBUSTION CHAMBER
HYDROGEN PUMP
TURBINE
TURBO-PUMPMOUNTING
OXYGEN INTAKE PIPE
STAINLESS STEEL SHELL
PAYLOAD
UPPER OXYGEN TANK
LOWER OXYGEN TANK
AUXILIARY OXYGEN TANK
INSULATION BULKHEAD
UPPER HYDROGEN TANK
HELIUM TANK
HEAT EXCHANGER
TURBO-PUMP COMPARTMENT
OXYGEN PUMP
HYDROGEN INTAKE PIPE
LOWER HYDROGEN TANK
AUXILIARY HYDROGEN TANK
CONTROL MOTOR FAIRING
HIGH ALTITUDE TEST VEHICLE
OVERALL LENGTH 77 feet
MAXIMUM DIAMETER 14.4 feet

FIG. [illegible]

The basic Martin rocket was an [illegible] [illegible] meters (77 feet) long and 4.4 meters (14.4 feet) in maximum diameter. The gross weight at launch was 46,[illegible] kg ([illegible] lbs), with a mass ratio of [illegible], including a [illegible] kg (1,450 lb) payload. Martin engineers projected the weight of the structural shell and bulkheads at 1[illegible],13[illegible] kg ([illegible] lbs), about [illegible] percent of the gross weight! [illegible] Medina assumed a west-to-east launching at equatorial latitudes to take advantage of the earth's peripheral velocity of rotation, together with a suitable number of ground stations located around the earth for tracking and data acquisition.

Unlike the North American prototype, the Martin vehicle employed no aerodynamic surfaces, and although the main stage Aerojet engine remained fixed, dynamic stability was to be provided by four auxiliary control engines which would damp oscillations in pitch, yaw, and roll. The pump-fed, transpiration cooled, liquid hydrogen-oxygen rocket engine, designed by David A. Young and Robert [illegible] at Aerojet, had an exit diameter of 4.1 meters (13 and one-half feet) and an overall length of 6.9 meters (22 and one-half feet) tapering to [illegible] meters ([illegible] feet) diameter at the chamber injection plate. The engine would provide [illegible] kg ([illegible] lbs) thrust at sea level varying to 13[illegible],000 kg (3[illegible] lbs) at altitude, with a specific impulse of [illegible] seconds. Its designers allotted [illegible] kg ([illegible] lbs) to the engine, turbopumps, valves and plumbing.[53]

[illegible] aware at this time that a small satellite could be made by multi-staging conventional (V-2) rockets, such as the Bumper WAC, the engineers at North American and

Martin responded to the Navy's requirements to see if a larger, single-stage satellite rocket could be devised. With the exception of the extensive work on the propulsion subsystem and some electronic devices, these studies were not hardware contracts. Rather, they involved preliminary feasibility and vehicle design studies for the purpose of evaluating the upper limit to which a single-stage liquid hydrogen-oxygen satellite rocket design could be carried. The answers were positive and encouraging: with appropriate extension of rocket engine burning times and with small improvements in metallurgy and electronics, a single-stage vehicle appeared definitely feasible.[54]

TERMINAL EVENTS

With the engineering designs completed in mid-1947, the BuAer satellite studies had progressed about as far as they could possibly go on paper. The prospect of a hiatus, or worse--complete cessation--of the work, caused individuals associated with the Navy proposal to seek again authorization of third phase hardware development and construction of a prototype vehicle. But the renewed search for institutional support would be made all the more difficult by the unresolved questions of satellite utility, the continued interest of the Army Air Forces in a separate satellite project of its own, and by the postwar reorganization of the armed services.

On January 24, 1947, Rear Admiral Leslie Stevens, Assistant Chief for Research and Development, BuAer, sent a letter to the Joint Research and Development Board (JRDB). Coordination of interservice requirements for earth satellites, he declared, was "beyond the scope of action" of the Aeronautical Board, particularly when one considered the possibility of mutual cooperation between civilian scientific and military groups.[55] The satellite held great potential for extending basic knowledge through cooperation with university scientists, Admiral Stevens continued, and he proposed that "the Joint Research and Development Board establish an agency for the coordination, evaluation, justification, and allocation of all phases of the Earth Satellite Vehicle program . . ." by means of a new ad hoc JRDB panel composed of civilian scientists as well as military representatives.[56]

The War Department's Joint Research and Development Board took Admiral Steven's proposal under advisement. Shortly thereafter, upon request of the Army Air Forces and in keeping with military comity extended in such situations, it was remanded to the Aeronautical Board for review before final action. Meantime, however, the American military services underwent the most profound reorganization in their collective history. On July 26, 1947, President Truman signed the National Military Act. A National Military Establishment, under a Secretary of Defense, replaced the historic Departments of War and Navy, and the Army, Navy and Air Force received equal service status. The Act also replaced the JRDB with a Research and Development Board (RDB).[57]

Speaking for the Aeronautical Board a few weeks before the Military reorganization, Air Force Lieutenant General Hoyt S. Vandenberg and Navy Vice Admiral D. B. Duncan informed the JRDB that, notwithstanding Admiral Steven's contentions concerning the interests of civilian scientists, the Aeronautical Board remained competent to decide matters of earth satellites. The JRDB, they said, would be apprised of any decisions made on the satellite program.[58]

Thus, throughout the latter half of 1947 the subject of earth satellites remained suspended in both governmental bureaus: in the satellite subcommittee of the Research and Development Committee of the Aeronautical Board, and in the JRDB-RDB. In the first group, progress on the Air Force and Navy satellite programs was reported and discussed, and other proposals on the disposition of responsibility for earth satellites considered. For example, late in the year the satellite subcommittee of the Aero Board "noted with interest" a second Navy proposal that offered the Office of Naval Research as a potential satellite coordinating agency in the view of the scientific aspects of the program. The subcommittee referred the issue to the full R&D Committee of the Aero Board "for action."[59]

Since the implications for basic scientific research in outer space rather obviously transcended military interests alone, the RDB likewise sought to decide where to place government responsibility for coordinating earth satellite activity.[60] At its initial meeting on December 19, 1947, the reconstituted Research and Development Board addressed this question and the Aeronautical Board's June response. Though the members judged a separate ad hoc coordinating committee originally urged by Admiral Stevens unnecessary,[61] they removed responsibility for satellite developments from the Aeronautical Board and vested it in the RDB Committee on Guided Missiles.[62] The Guided Missile Committee, in turn, assigned an evaluation of all the satellite work to one of its subpanels, the Technical Evaluation Group. The Committee instructed this civilian-staffed Technical Evaluation Group, chaired by Walter A. MacNair,[63] to provide an "opinion as to the desirability of such a [satellite] program, whether a single jointly supported program or separate competitive developments should be sponsored, and an estimate of the time scales and expenditures required for completion of the recommended program. . . ." by the end of March 1948.[64] With the log-jam over a coordinating agency broken at last, the way appeared open for Research and Development Board approval of continued Navy satellite work—or perhaps a joint program, if not with the Air Force, then possibly with scientific groups.

On March 29, 1948, the Technical Evaluation Group reported on its evaluation of the Navy and Air Force satellite programs. In a three-point opinion, the civilian group held the technical feasibility of building an earth satellite vehicle to be clearly established; however, neither the Navy nor the Air Force, the members declared, had as yet offered a military or scientific utility commensurate with the expected cost of such a vehicle. Consequently, the group recommended that at present, a satellite not be constructed. Because severe development problems could be anticipated in building a single-stage satellite rocket, the group further recommended that studies of the utility of earth satellites be the only serious activity to continue at Project RAND. Though the group encouraged the Navy to participate with the Air Force in the RAND studies and pursue limited development of liquid hydrogen-oxygen engines and lightweight tanks and structures,[65] the findings clearly spelled a deferral of satellite work in general, and, in particular, an end to the Navy's single-stage satellite rocket.

A month later in April, Harvey Hall and his associates in BuAer made a last desperate attempt to continue the Navy satellite program in modified form. This die-hard group of space scientists now urged construction of liquid hydrogen-oxygen "Interim Test Vehicle, M07," reconfigured to a super performance sounding rocket that would rise to an altitude of 320-640 km (200-to-400 miles) (Figure 3). They submitted this proposal[66] to an ad hoc subcommittee of the National Advisory Committee for Aeronautics (NACA) that had been recently established to explore and organize methods for extending the tables of the standard atmosphere at altitudes above 6,100 meters (20,000 feet).[67] If approved and built, the Navy satellite proponents hoped the sounding rocket might rally eventual financial support for the construction and launch of an earth satellite vehicle.

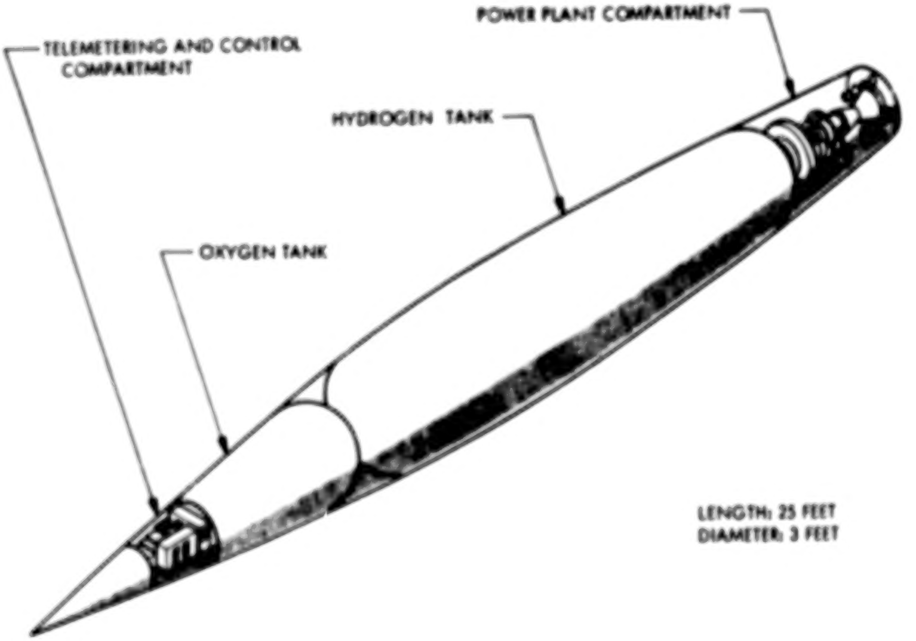

Fig. 3

In addition to the NACA Subcommittee on the Upper Atmosphere, Navy proponents also sought formal acceptance and endorsement of the new rocket from the RDB's Geophysical Sciences Committee. By this time, however, the BuAer proposal confronted severe obstacles--not only in the form of the Technical Evaluation Group findings of the previous month, but in direct competition from a companion at arms: the Office of Naval Research had already received Navy approval to construct and launch a sounding rocket for upper atmosphere research.[68] According to Hall, the response to this penultimate proposal ended all Navy satellite studies at this time. The RDB Geophysical Sciences Committee took no action whatever; the NACA Subcommittee on the Upper Atmosphere favorably endorsed the project but had no funds to commit to the work. Whatever interest in earth satellites that existed at higher command levels in the Navy had now all but disappeared.[69]

In March 1948, in response to a Presidential ceiling on expenditures for fiscal year 1949, the Navy Bureau of Aeronautics began transferring the remaining funds earmarked for satellites to other projects of more immediate interest. The Navy satellite effort terminated for all practical purposes on June 22, 1948, when the Pilotless Aircraft Division notified Admiral Stevens that "the Earth Satellite Project has been discontinued as such and work is now proceeding with emphasis on the development of a liquid-oxygen, liquid-hydrogen rocket engine. When this power plant appears feasible, flight test vehicles will be designed and constructed."[70]

At Aerojet, work on this last remaining satellite component did continue for one more year, but suffered from technical problems associated with engine heat exchange and chamber cooling processes. That effort concluded in 1949,[71] ringing down the curtain on Navy satellite efforts in the 1940s. The total cost to the Navy of all satellite-related work conducted between 1945 and 1949 has been estimated at approximately $1.5 million.[72] Ironically, when the satellite program expired in the BuAer Pilotless Aircraft Division, the Navy reprogrammed the remaining funds (around $170,000) to the Office of Naval Research for development of the Viking sounding rocket, also under contract at the Glenn L. Martin Company.[73] In time, Viking became the first stage booster for America's International Geophysical Year satellite in the mid-1950s.[+]

CONCLUSION

The earth satellite studies undertaken by the Navy Bureau of Aeronautics in the late phases of World War II responded to (1) rapid wartime advances in rocket technology at home and abroad, (2) an encounter between members of an Allied technical intelligence team and German rocket specialists from the Peenemunde rocket base, and (3) accounts of the V-2 ballistic missile and its potential applications published in open literature

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[+] See M. W. Rosen, "The Viking Rocket: A Memoir," in this volume - Ed.

and in contemporary intelligence reports. These investigations took place before the invention of the transistor, the development of solid-state circuitry,[74] and the spread of digital computers; they were complicated further by questions of satellite utility, military jurisdiction, the technical difficulties involved in a single-stage, hydrogen-oxygen satellite rocket, and the projected costs and economies of the moment. It is reasonable to assume that one or a combination of all these conditions would have deterred the endeavor in the years that immediately followed. The participant's inability to secure Navy support outside BuAer, added to these factors, prefigured the denouement.[75]

Before March 1948, a Navy decision to proceed to third-phase construction of a prototype hydrogen-oxygen single-stage satellite vehicle was, ultimately, Rear Admiral Stevens' to make. The Admiral, a man of strong convictions, surmised that a third-phase hardware program in 1947-1948 would involve a technical undertaking of colossal proportions, with little prospect for any early return on the investment. Without broad internal interest or joint service sponsorship for a satellite program, he elected not to proceed.[76] If keen enthusiasm remained evident throughout this period among individuals in its research bureaus, influential members of the operating arm of the Navy remained generally unaware of--or at least not much taken with--these studies or the satellite's potential applications.[77] The demise of Navy-BuAer satellite studies in the late 1940s appear to devolve from these two interrelated causes: reserved institutional support (which is normally reflected in priorities), and contemporary exigencies associated with technology, cost, and vehicle utility.

On the other hand, the Air Force--a relatively new service primarily concerned with flight in and beyond the atmosphere--moved actively into satellite research. Prompted by the Navy work, the Air Force soon issued orders for active institutional support of an earth satellite program at the highest levels of command.[78] This interest was sustained, waited on or advanced the required technology, and culminated in the Thor-Agena and Atlas-Agena satellite flights that began eleven years later, in 1959.

Though the aspirations of Haviland, Hall, and their Navy colleagues would not be fulfilled as rapidly as they wished, an historic milestone had passed for astronautics in the late 1940s. The technical feasibility of space flight--as opposed to the theoretical feasibility of this activity developed by Goddard, Tsiolkovsky, Oberth and others during the early years of this century--had been firmly established and officially acknowledged.[79] Questions asked and answered would no longer remain focused on astronautical theory,[80] but on practicalities of when and how, and on satellite utility. Answers to these questions would come in the decade to follow.

ACKNOWLEDGMENTS

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Robert P. Haviland and Harvey Hall; Myra Grenier, Corporate Librarian, Aerojet General Corporation; Eugene M. Emme, NASA Historian; Lee M. Pearson, Historian, Naval Air Systems Command; D. C. Allard, Operational Archives Branch, Office of the Chief of Naval Operations; Max Rosenberg, Deputy Chief Historian, Office of the Air Force Chief of Staff; R. A. Winnaker, Historian, Office of the Secretary of Defense; and Herman G. Goldbeck and Thomas E. Hohmann of the General Services Administration, National Archives and Records Service.

REFERENCES

1. Military ranks were assigned civilians functioning in military zones. Commissioned an Army Lieutenant, Dr. Hsue-shen Tsien was a protege of Theodore von Kármán at the California Institute of Technology and, as a student, a member of the GALCIT Rocket Research Project during 1937-1939.

2. Events as reconstructed in a telephone interview, Abraham Hyatt with the author, January 22, 1970. While not present at this meeting Hyatt related that Tsien informed him of the proceedings during subsequent discussions. Von Braun's paper was published in F. Zwicky, *Report on Certain Phases of War Research in Germany*, Vol. 1, Aerojet Engineering Corporation, October 1, 1945, 66-72; republished by the Air Materiel Command, Wright Field, Dayton, Ohio, in January 1947.

3. Cf., Hermann Oberth, *Die Rakete zu den Planetenraumen*, Verlag von R. Oldenbourg, Munich and Berlin, 1923, NASA TT F-9227, pp. 94-96; and H. Noordung, *Das Problem der Befahrung des Weltraums*, Berlin, 1929; on German wartime planning, see Theodore von Kármán, *Where We Stand*, a report prepared for the AAF Scientific Advisory Group, August 1945, [published May 1946 by Headquarters, Air Materiel Command] 13; also, "The Station in Space," *Journal of the American Rocket Society*, #63, September 1945, 8; and Zwicky, *loc. cit*.

4. Hyatt Interview, *loc. cit*.

5. Haviland, who demonstrated a youthful interest in electronics, graduated from the Missouri School of Mines in electrical engineering in the late 1930s together with a fellow classmate in the same field, George Mueller, who would later serve as NASA Associate Administrator for Manned Space Flight. (R. P. Haviland, "Personal Profile," *Journal of Space Flight*, March, 1970, 106 ff.), Capt. W. P. Cogswell, an associate at BuAer, recalled the lieutenant's unrestrained enthusiasm for astronautics years later: "More than once Haviland told me: 'Captain, you've got to get space-minded!'" (Letter, Captain W. P. Cogswell, USN (Ret.) to the author, February 9, 1970.)

6. The material reviewed by Haviland at this time is referenced in his satellite memo cited at note 7. Much later Haviland recalled that he had also seen a letter to the editor from RAF radarman Arthur C. Clarke which appeared in the British Journal *Wireless World*. The Clarke letter proposed the postwar use of V-2 rockets for (1) exploring the upper atmosphere as rocket sondes, and (2), with upper stages, placing unmanned satellites

in orbit about the earth to conduct scientific experiments and to act as communications relay stations. If accurate, this letter and the work by Willy Ley apparently started Haviland thinking seriously about astronautics several months earlier. Telephone interview with Robert P. Haviland, January 28 and June 19, 1969. (See also, Clarke, "V-2 for Ionosphere Research?" in Letters to Editor, Wireless World, February 1945, p. 58.)

7. Memo Aer-E-313-RPH, F.41 (1), from R. P. Haviland, Beacon Desk, Identification Section, Elect. Materiel Branch, Subject: Comments Concerning Use and Development of Rockets, August 10, 1945, 8. Knowledge of the contemporary German suggestions is indicated when he ". . . noted that the German Command was actually planning such a [satellite] program with the added feature of an impractical 'Sun Gun.'"

8. Chambers agreed that "space-ships, as proposed [by Haviland] will afford a world-wide navigation and communication system on high frequencies, and free from horizon limitations and sky-wave errors." Memo, Aer-E-315-JAC, F41 (1), from Head, Special Weapons Section, to Head, Experiments and Developments Branch, Subject: "First Endorsement on Beacon Desk . . . Memo Aer-E-313-RPH F41 (1) Written 10 August 1945," August 25, 1945.

9. Letter, Captain W. P. Cogswell, loc. cit. Dr. Berkner apparently was first attracted to the ionosphere as an area of research in 1937; satellites no doubt appeared as a practical means to extended investigations in this region. In the early 1950s Dr. Berkner was instrumental in the creation of the U.S. IGY satellite program. Years later he intimated that a still earlier satellite technology survey may have been conducted at BuAer: "During the war our early studies in the Navy showed the orbiting Earth satellite was within the range of our technology. But in 1943 it was clear that space technology, aside from short-range rockets, would not be a factor in that war, so the matter was laid aside. . . ." (Italics added, Lloyd V. Berkner, "Science, the Scientist and Space," Proceedings of the Conference on Space Science and Space Law, June 18, 1963, ed. by Mortimer D. Schwartz, New Jersey: Fred B. Rothman & Co., 1964, 1.) Dr. Harvey Hall, science advisor to Dr. Berkner between 1941 and 1944 has indicated, however, that these earlier wartime studies concerned rockets, not satellites. Letter, Harvey Hall to the author, December 4, 1969.

10. D. S. Fahrney, "The History of Pilotless Aircraft and Guided Missiles," BuAer, 1958, unpublished, p. 1132.

11. Dr. Hall, a theoretical physicist, had just returned from an assignment with the U.S. Naval Technical Mission in Europe during August 1945, and had been persuaded to remain in the Navy by Rear Admiral Leslie Stevens to assist in evaluating the future of jet propulsion primarily with respect to its potential value for naval aviation. Admiral Stevens was concerned that with the sharply reduced budget, the Navy might "find itself irreparably frozen in an absolute pattern in aviation." (Hall letter, December 4, 1969, loc. cit.) While Lt. Robert Haviland provided the initial spark setting earth satellite events in motion, Dr. Harvey Hall, the theoretician, largely determined the technological course these events were to take at BuAer in succeeding months.

12. "Not that orbiting payloads was impossible without it [the advance], but the rockets would be large, complex, expensive, with small payloads, and the missions that had been proposed . . . hardly appeared to justify the effort." Harvey Hall, letter of December 4, 1969, loc. cit.

13. Memo, Aer-E-203-KWM, from Captain R. S. Hatcher, Deputy Director of Engineering, Experiments and Developments, to Distribution, Subject: Establishment of a Committee for Evaluating the Feasibility of Space Rocketry, October 3, 1945.

14. "Minutes of the Fourth Meeting of the Space Rocket Committee," October 29, 1945, Lt. (jg) Max, Secretary. Members of the satellite committee were:

Comdr. C. D. Case - E&D
Lt. (jg) K. W. Max - E&D
Comdr. H. Hall - R&E
Lt. R. P. Haviland - R&E
Lt. F. A. Parker - Sh. Instal.
Lt. I A. Hanson - Sh. Instal.
Lt. Comdr. O. E. Lancaster - ADR
Mr. J. R. Moore - ADR
Mr. H. Driggs - ADR

(Driggs attended meetings but was not a formal member of the committee.)

15. During the next three years participants in the BuAer satellite project referred to their work variously as the Earth Satellite Vehicle (ESV), Project Orbit, as well as by an eminently respectable term, High Altitude Test Vehicle (HATV). All of these Navy studies, beginning with the satellite committee, considered only unmanned satellites, in contrast to the original Haviland proposal.

16. O. E. Lancaster, J. R. Moore, P. B. Olmsted, and M. M. Taylor, Investigation on the Possibility of Establishing a Space Ship in an Orbit Above the Surface of the Earth, ADR Report R-48, November 1945. This document is pessimistic about the possibility of realizing a single-stage vehicle, and Harvey Hall appears to have remained the champion of this particular approach. (Cf., Hall's proposal for a single-stage vehicle; an enclosure to the Minutes of the Third Meeting of the CEFSR, October 22, 1945.)

17. Captain Cogswell recollects tempering support for the satellite vehicle, which he considered a deserving project, with skepticism concerning the mass-ratio involved in a single-stage satellite rocket burning a propellant combination that "was of course only a dream at the time." (Cogswell letter, loc. cit.)

18. D. S. Fahrney, op. cit., 1132; also telephone interview with R. P. Haviland June 19, 1969. (See also note 34 below.) The duties of the BuAer Office of Engineering for Experiments and Developments, headed by Captains T. C. Lonnquest and R. S. Hatcher, were expanded and divided by Rear Admiral Stevens when he arrived at BuAer in February 1946. Captain C. A. Nicholson became Assistant Chief for Design and Engineering, BuAer, while Rear Admiral Stevens became Assistant Chief for Research and Development, BuAer. "In this reshuffle Rear Admiral Stevens took a much more active interest in R&D affairs than had been possible for Captain Lonnquest. . . ." (Rear Admiral R. S. Hatcher, USN (Ret.), letter to the author, July 3, 1970.)

19. Fahrney, op. cit., 1134. During the next few years there were no more than ten-to-fifteen BuAer personnel actively involved in the satellite project. For the most part these individuals guided and monitored the activities of the various participating contractors, where many more people were involved.

20. R. P. Haviland, "Notes on the Earth Satellite Vehicle," The Guided Missile, July 1946. (The Guided Missile was an in-house publication of the Guided Missiles Committee, Joint Committee on New Weapons and Equipment, Joint Chief of Staff.) In concluding this article, Haviland observed: "The full effect of this [satellite] development on military operations and indeed on world conditions in general, can hardly be forecast. Its possibilities as a peacetime aid to rocket travel and communications is unlimited. The writer is of the opinion that interplanetary travel will become possible, perhaps even commonplace, within twenty-five years." He was not far wrong.

21. This work, authorized in December 1945 as a task under Contract NOa(s) 5350, began investigations of the use of hydrogen as a fuel in jet engines. Memo Aer-PA-52 F41(1) from Rear Admiral L. C. Stevens, for Chief, Bureau of Aeronautics, to Joint Research and Development Board, Subject: Agency for Coordination of Earth Satellite Vehicle, January 24, 1947, 1, 2.

22. Aerojet Engineering Corporation was a rocket production firm founded during World War II by members of the GALCIT Rocket Research Project at the California Institute of Technology to fabricate rocket motors for the armed services. Although Caltech actively sponsored rocket research in the United States during the 1930s, it held no interest in large-scale manufacturing operations. The GALCIT Rocket Research Project eventually expanded to become the Jet Propulsion Laboratory in 1944, a non-profit research and development facility operated by the California Institute of Technology under contract to the government. See F. J. Malina, "On the GALCIT Rocket Research Project, 1936-1938," in the Proceedings of the First and Second History Symposia of the International Academy of Astronautics, *Smithsonian Annals of Flight*, No. 10, 1974, pp. 113ff; also, Malina, "Origins and First Decade of the Jet Propulsion Laboratory," in Emme, ed., *The History of Rocket Technology* (Detroit: Wayne State University Press, 1964).

23. The availability of this hydrogen liquefier was later to fill an important requirement in the AEC's hydrogen bomb development program, after the Navy Bureau of Aeronautic's hydrogen rocket program had been terminated.

24. Interview with Harvey Hall on Early Navy Satellite Studies, March 31, 1963. Lithaloys Corporation had earlier begun work on means of producing experimental quantities of lithium-borohydride for another project.

25. Memo, Aer-PA-52 F41(1), to Joint Research and Development Board, January 24, 1947, *op. cit.*, 1; also, BuAer Contract NOa(s)7913 with the California Institute of Technology, dated December 12, 1945. Dr. Hall had already examined the effect of aerodynamic drag during ascent to earth orbit because, even though it was only a correction factor, it was an important consideration for the proposed lightweight structure in terms of potential deformation of shape and extra propellant required to overcome the drag. Since no test work had been done at such high aerodynamic speeds, and the theory of supersonic flight was not yet developed, "I again turned to basic theory and found that, even assuming the worst aerodynamic shape possible, the effect of drag would amount at most to a few percent." (Harvey Hall letter, December 4, 1969, *loc. cit.*) Hall's major contribution at this time was in devising a method of approximate analysis for ascent trajectories which greatly reduced tedious step by step mathematical integration in arriving at orbit computations--necessary in an era before electronic computers. (Telephone interview with R. P. Haviland, May 25, 1970; theory in Harvey Hall, "Estimation of Mass Ratio for Rocket Ascent," October 1945.

26. Cited in H. J. Stewart, *A Summary of Performance Studies for a High Altitude Orbiting Missile*, JPL Report No. 8-5, July 10, 1946, 1. See also Aerojet Engineering Corporation, *Calculated Performance of Hydrogen and Oxygen as Jet Motor Propellants*, Report No. RTM-24, February 28, 1946; and, D. A. Young, C. C. Stitler, and H. Coplen, *Research and Development of High Performance Rocket Motors Using Gaseous Hydrogen and Oxygen as Propellants*, Project Final Report, Aerojet Report R-64, August 26, 1946.

27. JPL Report No. 8-5, *op. cit.*, 11. This idea first appears in Hall, "Estimation of Mass Ratio for Rocket Ascent," and forms a premise for figuring ascent trajectory calculations in Lancaster, et. al., ADR Report R-48, November 1945, *op. cit.*, 14. Considering this development many years later, Hall remembered using the concept in configuring single-stage trajectories, but "it may well have been originated by others also," he concluded. "In fact, I did not regard it as anything especially clever at the time, except that it supported the single-stage concept. . . . Anyone studying these problems would surely be led before long to this scheme." Harvey Hall letter, December 4, 1969, *loc. cit.*

28. They are W. Z. Chien, *Preliminary Calculations on the Performance of a High Altitude Test Vehicle*, JPL Memorandum No. 8-1, January 3, 1946; Chien, *Studies on the Performance of a High Altitude Test Vehicle with Uniform Burning Rate Based on Vertical Trajectories as the First Approximation*, JPL Report No. 8-1, March 18, 1946; E. C. Sledge and G. G. Halvorson, *Investigation of the Launching Trajectories of a High Altitude Test Vehicle with Uniform Burning Rate*, JPL Report No. 8-2, June 18, 1946; Joseph V. Charyk, *Studies of the Effects of Nonuniform Burning Rates on the Performance of a High Altitude Test Vehicle*, JPL Report No. 8-3, July 3, 1946; W. Z. Chien, *Studies of a High Altitude Test Vehicle in a Dissipating Elliptic Orbit*, JPL Report No. 8-4, July 9, 1946; and H. J. Stewart, JPL Report No. 8-5, at *op. cit*.

29. JPL Report No. 8-5, *op. cit*., 3. According to Dr. Malina, first director of JPL, at about the same time as the BuAer project began, the study of sounding rockets (first initiated by the GALCIT Rocket Research Project in the late 1930s) was resumed and extended to include step rockets and satellites. (Letter, F. J. Malina to the author, December 8, 1969.) See also F. J. Malina and A. M. O. Smith, "Flight Analysis of the Sounding Rocket," *Journal of the Aeronautical Sciences*, No. 6, 50 (1938); F. J. Malina, "Is the Sky the Limit? Ordnance Rockets Demolish the Barriers of Space," *Army Ordnance*, July 1946 (a recommendation for staging the JPL WAC Corporal sounding rocket atop German V-2 missiles for upper atmosphere research); and, F. J. Malina and Martin Summerfield, "The Problem of Escape from the Earth by Rocket," JPL Publication No. 5, August 23, 1946 (theoretical analysis of requirements involved in projecting a rocket to escape velocity).

30. Contract NOa(s)8496 discussed in Fahrney, *History of Pilotless Aircraft and Guided Missiles*, *op. cit*., 1133-1134, and letter from L. M. Pearson, Naval Historian, to the author, AIR-06B-LMP, July 29, 1969. Subsequently, on June 27, 1947 as design of the full-scale engine was completed--and under an amendment to the same contract--BuAer authorized Aerojet to develop and fabricate a liquid hydrogen-oxygen rocket engine of 2,000 to 3,000 pounds thrust, essentially a small scale experimental model of the anticipated, final version.

31. See D. A. Young, Progress Report, Aerojet R-70, January 1947, NOa(s)8496; and, Robert Gordon, *Final Report on 300,000 Pound Thrust Liquid Rocket Power Plant*, Aerojet R-74, March 1947; also, Robert Gordon, *The Relative Importance of Propellant Density and Specific Impulse on Satellite Vehicle Performance*, Aerojet R-92, May 1948.

32. The power unit, with an estimated weight of about 13 pounds, was to employ hydrogen as a working medium and use a liquid such as methanol as a heat exchanger. See Harvey Hall, "Early History and Background on Earth Satellites," ONR memo, ONR:405:HH:dr, November 29, 1957, 3; and, letter from L. M. Pearson, AIR06B:LMP, December 30, 1969. Hall indicates that mass ratio constraints favored a "small but useful power output of about 30 watts . . . [over] a higher power but heavier nuclear energy or nuclear isotope power source," which would require a more extensive development period.

33. BuAer Memo Aer-E-31T-HH, CX-281360, November 27, 1945, cited in Harvey Hall memo, "Early History and Background on Earth Satellites," *loc. cit*.

34. As one observer recalled: "After V-J day . . . scientists, as at the Radiation Lab., could not be interested in guided missile projects (of which the communications satellite [ESV] was only one) and wanted only to wind up their affairs and go back to their former pursuits, or better. Likewise the BuAer admirals were unenchanted by guided missile projects--as one remarked: 'We want equipment that we know works '" (Letter, Capt. W. P. Cogswell, *loc. cit*.); and, Albion and Connery point out that "Fiscal 1946, drawn up in anticipation of a huge invasion of Japan, quickly began to be whittled away before it could be spent. Altogether, more than $20 billion of Navy funds were cut back." Fiscal 1947 was finally approved at $4.1 billion (as opposed to a Navy request for $6.3 billion) and Fiscal 1948 and 1949 followed at $3.6 and $3.7 billion respectively. See R. G. Albion and R. H. Connery, *Forrestal and the Navy* (New York: Columbia University Press, 1962) 154.

35. BuAer letter to Commanding General, Army Air Forces, Aer-EL-1-HH, F 41(1), Ser. C02262, March 15, 1946; cited in Harvey Hall memo, "Early History," 1957, loc cit. Members attending the conference of March 7 were: Major General H. J. Knerr, AAF; Major General H. M. McClelland, AAF; Brigadier General W. L. Richardson, AAF; Captain W. P. Cogswell, USN; Commander H. Hall, USNR.

36. Interview with Harvey Hall, March 21, 1963, loc. cit.

37. BuAer memo Aer-PA-52 F41(1), to Joint Research and Development Board, January 24, 1947, op. cit., p. 2.

38. In his Third Report to the Secretary of War the preceding year, General H. H. Arnold forecast that, as antiaircraft weapons were perfected, strategic bombers eventually would be replaced by long-range V-2 type ballistic rockets. He continued: "If defenses which can cope even with a 3,000 mile-per-hour projectile are developed, we must be ready to launch such projectiles near the target, to give them a shorter time of flight and make them harder to detect. We must be ready to launch them from unexpected directions. This can be done from true space ships, capable of operating outside the Earth's atmosphere. The design of such a ship is all but practicable today; research will unquestionably bring it into being within the foreseeable future." Arnold, Third Report of the Commanding General of the Army Air Forces to the Secretary of War, November 12, 1945, "New Concepts," p. 68. Moreover, in the Foreword to its report on a Proposed Air Engineering Development Center (eventually the Arnold Engineering Development Center in Tennessee), released on December 10, 1945, the AAF had asserted that planning for the new experimental facility must include "the expeditious and continuous development of all materiel contributing to air power in its broadest sense." This involved "1. Supersonic aircraft, piloted and pilotless; and winged missiles having velocities approaching meteoric velocities," and "2. Flight and survival equipment for use above the atmosphere, including space vehicles, space bases, and persuasive devices for use therein." Engineering Division, Air Technical Service Command, report, Proposed Air Engineering Development Center, December 10, 1945, p. iv.

39. Project RAND was chartered by the Army Air Forces in March 1946, to pursue a program of study and research devoted to the broad subject of intercontinental strategy and warfare. Contract W-33-038ac 14105 formalized this effort in May 1946. Initially, Project RAND operated as a semi-autonomous branch of the Douglas Aircraft Corporation, responsible directly to the Vice President, Engineering. Qualified physicists, engineers, and mathematical analysts were drawn from Douglas and other nearby aircraft firms as well as universities, and settled at Santa Monica, California, in separate quarters. In November 1948, the RAND Corporation was created and became a completely autonomous, non-profit research institution.

40. Formal members of the Aeroboard Research and Development Committee at this ime were Brigadier General Laurence C. Craigie, Army Air Forces, Colonel H. G. Bunker, A. C.; Rear Admiral Leslie C. Stevens, U.S.N., Chairman; and Captain R. S. Hatcher, U.S.N. Also present at Research and Development meetings were additional invited personnel.

41. Adrian O. Van Wyen, The Aeronautical Board, 1916-1947 (Washington, D.C.: Deputy Chief of Naval Operations (Air), History Unit, GPO, 1947), 1. Later, in June 1946, this organization was partially supplanted by the Joint Research and Development Board [JRDB] of the War Department chaired by Vannevar Bush, which expanded and formalized many of the functions performed by the Aeronautical Board over a wider spectrum of activities. (The Aeronautical Board passed away completely in 1948.)

The chief responsibility of the JRDB involved preparation of an integrated program of research and development, in the light of which individual projects of the Army, Navy, and Air Force could be evaluated. The Board decided who developed what weapons. It made sure that there was no unnecessary duplication in the activities of the services, although it could permit competition which promised to produce better results. JRDB in turn was superseded by the RDB in 1947. Because service assignment of responsibility for different range rocket weapons could not be agreed upon until years later, by the late 1940s the principle of preventing duplication of effort among the services had been seriously eroded.

42. Harvey Hall memo, "Early History and Background on Earth Satellites," *op. cit.*, 4.

43. Research and Development Committee, Aeronautical Board, Case No. 244, Report No. 1, May 15, 1946, pp. 1-2.

44. *Ibid.*, p. 4.

45. In subsequent meetings of this ESV subcommittee in 1946-1947 a firm, but unwritten, understanding was reached between Navy and Army Air Forces representatives which divided the field between the two technical approaches: single-stage to the Navy, and multi-stage to the Army Air Forces. (Rear Admiral R. S. Hatcher, letter to the author, *op. cit.*, p. 4.) p. 4.)

46. The CNO affirmed that,"inasmuch as an earth satellite vehicle will probably contribute importantly to the advancement of knowledge in the fields of guided missiles, communications, meteorology, and other technical fields with military applications, the program as set forth is approved." Letter CNO to BuAer, Ser. 037P517, May 8, 1946, cited in "Status Report, Earth Satellite Vehicle," Committee on Guided Missiles, Research and Development Board, GM 13/5, 15 January 1948, 4.

47. Initial discussions were conducted with several airframe manufacturers in early March 1946; and contracts were awarded after the Chief of Naval Operations approved the design studies on May 8. (Cf., Douglas Aircraft Co., Inc., Report No. E.S. 20515, "Considerations of a High Altitude Space Vehicle (Hall Project)," March 28, 1946; and, Glenn L. Martin Co., Report No. 2373, same subject, May 1946. North American Aviation prepared an informal summary of its ESV considerations. Douglas later withdrew from participation in the Navy ESV design study because of its close affiliation with Project RAND satellite studies being conducted for the Army Air Forces at that time.

48. G. Wilson, *Structural Design Study, High Altitude Test Vehicle*, Contract NOa(s) 8349, North American Aviation, Inc., Report No. NA 46-758, September 26, 1946, p. 1-1.

49. *Ibid.*, pp. 2-1 and 3-1.

50. Ultimately the United States Air Force approximated just what BuAer had proposed when an entire Atlas ICBM was guided into orbit in late 1958 in "Project Score." As finally developed, the "stage and a half" Atlas ICBM was also a thin-skin, stainless steel, pressure-stabilized structure; moreover, the Atlas employed conventional propellants (kerosene and liquid oxygen) which do not approach the higher specific impulse delivered by liquid hydrogen-oxygen, in some ways making that vehicle a more difficult engineering achievement than the original Navy Bureau of Aeronautics single-stage HATV satellite rocket proposal.

51. Pedro C. Medina, *Progress Report No. 1, Orbit Project*, Contract NOa(s) 8376, The Glenn L. Martin Company, Report No. 2454, September 15, 1946, p. 5.

52. Pedro C. Medina, *HATV Summary Report*, Contract NOa(s) 8376, The Glenn L. Martin Company, Report No. 2666, June 1947, pp. A and B.

53. R. Gordon, D. A. Young, G. Bosco, and A. Pelton, *Final Report on a 300,000 Pound Thrust Liquid Rocket Power Plant*, Contract NOa(s) 8496, Aerojet Engineering Corporation Report No. R-74, March 31, 1947, pp. 2 and 4.

54. In addition to the foregoing design studies, the Navy also found confirmation of this concept in two more limited separate studies, the first--already mentioned, ADR Report No. 48--prepared by the BuAer Aviation Design Research group under the technical direction of I. H. Driggs, and the second by W. F. Ballhaus at the Douglas Aircraft Company. Letter, O. E. Lancaster to the author, February 6, 1970; and W. F. Ballhaus, Preliminary Design of a Satellite, Douglas Aircraft Company, Report No. ES 20636, August 7, 1946.

55. The Aeronautical Board, comprised of military personnel, did not contain civilian representatives.

56. Memo Aer-PA-52 F41(1), Stevens to Joint Research and Development Board, January 24, 1947, op. cit., 3.

57. As provided for in the legislation, the Research and Development Board (RDB) was established effective September 30, 1947, to assist the Secretary of Defense in coordinating the R&D activities of the armed services. Vannevar Bush also chaired the new RDB, which absorbed and superseded the older JRDB (formed in June 1946). RDB responsibilities were defined to include determination of availability and approval of equipment developed for use by the military. (Responsibility for procurement, production, and supply of approved equipment continued to reside in the Munitions Board, while the Joint Chiefs of Staff remained in charge of operations.)

58. Memo for the JRDB from Hoyt S. Vandenberg, Lt. Gen. USAF, Subject: Agency for the Coordination of Earth Satellite Vehicle, June 13, 1947; also, Report No. 2, Case No. 244, The Aeronautical Board Research and Development Committee, Memo for the Aeronautical Board, June 13, 1947, 2.

59. Cited in Fahrney, op. cit., 1140. Since the predisposition of the services in the full committee was already known, this referral amounted to tabling the motion.

60. In April 1947, as impediments to further satellite progress grew (i.e., questions of jurisdiction, satellite utility, and limited funding and support) Lt. Commander R. P. Haviland requested orders to inactive duty, and subsequently joined the General Electric Corporation's Hermes rocket development project. He has remained with G.E., involved in work on various space programs since that time.

61. Farhney, op. cit., 1140-1141.

62. Memo to the Committee on Guided Missiles, from L. R. Hafstad, Executive Secretary of the RDB, Subject: Earth Satellite Vehicle, GM 13/4, January 7, 1948; and, Fahrney, loc. cit.

63. Dr. Walter A. MacNair of the Bell Telephone Laboratories, served as chairman of this six-man civilian panel in the late 1940s. The other members were Dr. Francis Clauser, RAND Corporation; Dr. Richard Porter, General Electric Company; Mr. Robert L. Gilruth, NACA; Dr. H. Guyford Stever, Massachusetts Institute of Technology; and Dr. Clark Millikan, California Institute of Technology.

64. Letter from Frederick L. Hovde, Chairman of the Committee on Guided Missiles, RDB, to W. A. MacNair, Bell Telephone Laboratories, February 6, 1948.

65. "Satellite Vehicle Program," Technical Evaluation Group, Committee on Guided Missiles, Research and Development Board, GM 13/7, MEG 24/1, March 29, 1948. The first public announcement of contemporary U.S. earth satellite programs, contained in the First Annual Report of the Secretary of Defense, 1948, evoked some controversy in this country as well as in the Soviet Union. (R. Cargill Hall, "Early U.S. Satellite Proposals," Technology and Culture, Vol. IV, No. 4, 1963, 427-428.)

66. The Glenn L. Martin Company, Engineering Report No. 2854, Model M07, Proposed Interim Test Vehicle, March 1948.

67. The data was needed to provide aircraft firms information on pressures, atmospheric drag and heating effects at these high altitudes for use in the design of advanced jet aircraft. Subcommittee members included Captain W. S. Diehl, USN; Dr. Homer E. Newell, Naval Research Laboratory; Dr. Fred Whipple, Smithsonian Observatory; Harry Wechsler, U.S. Weather Bureau; and Dr. Harvey Hall, USNR; among others.

68. The Office of Naval Research smaller Neptune rocket, burning conventional propellants and carrying the same name as a contemporary airplane then under design for the Navy, was rechristened "Viking" to avoid anticipated confusion. Evolution noted in M. W. Rosen, The Viking Rocket Story (New York: Harper & Brothers, Publishers, 1955).

69. Harvey Hall, "Early History and Background on Earth Satellites," op. cit., p. 4.

70. BuAer PAD Memo, Aer-PA-5, June 22, 1948, cited in Fahrney, op. cit., 1141-1142. Dr. Harvey Hall left the Navy a few months later, worked for a time on earth satellites with the RAND Corporation in Santa Monica, California, and then resumed a teaching career at the University of Southern California. He joined the United States space program in 1960, after the creation of the National Aeronautics and Space Administration, serving NASA as Chief Scientist, Advanced Manned Missions Programs.

71. Aerojet Engineering Corporation, Research Technical Memo No. 54, Development of a 300-lb. Thrust Liquid-Hydrogen — Liquid-Oxygen Rocket Motor, August 29, 1949, 1.

72. Letter, AIR-06B-LMP, from Lee Pearson, Naval Historian, to the author, July 29, 1969; and Hall, "Early History . . .," loc. cit.

73. Fahrney, op. cit., 1142. (See also note 68, above.)

74. These technical circumstances account for the large 1000-lb. payload that was stipulated for the ESV in the Navy design studies in 1946-1947.

75. Perhaps the lack of interest may be attributed to the Navy's historic preoccupation with surface vessels and life that surrounds this singular activity. See Elting E. Morison, Men, Machines, and Modern Times (Cambridge, Mass.: MIT Press, 1966) especially Chapters 2 and 6.

76. It should be noted that, as the budget tightened after World War II, the Navy curtailed a number of surface-oriented R&D projects in aircraft and submarines, to the point of eliminating anti-ship missiles and light gun barrels and mounts, for example. Admiral Stevens thus had to justify major funding for a space effort at a time when it would mean cutting back still further on projects directly involved "with fulfillment of the Navy's responsibilities on, over, and under the sea." Early cost estimates for construction and launch of a Navy prototype vehicle were set at $150 million (not including launch facilities), a figure, considering the LOX-hydrogen applications, that was probably underestimated considerably. In short, it just was not a salable proposition in the 1940s. (See also note 34, above.) R. P. Haviland, telephone interview with the author, July 2, 1970. A similar observation concerning Admiral Stevens' view of the expected cost and technical difficulties is contained in Rear Admiral R. S. Hatcher, letter to the author, loc. cit.

77. The single notable exception to this observation was CNO approval of BuAer satellite design studies on May 8, 1946. (See note 46, above.) One of the ESV participants, Grayson Merrill, recently observed: "As it turned out space flight was of very limited application to the Navy compared to other government agencies and, I think, the Navy 'old timers' sensed this." Capt. Grayson Merrill, USN (Ret.) letter to the author, July 21, 1970.

78. On January 15, 1948 the USAF issued an earth satellite policy which declared that, because of the strategic implications, logical responsibility for satellite developments belonged to the Air Force. The policy "states, 'research and development will be pursued as rapidly as progress in the guided missiles art justifies and requirements dictate. To this end the problem will be continually studied with a view to keeping an optimum design abreast of the art, to determine the military worth of the vehicle, considering its utility and probable cost--to insure development in critical components, if indicated--and to recommend initiation of the development phases of the project at the proper time.'" (*Statement of Policy for a Satellite Vehicle*, General Hoyt S. Vandenberg, USAF Vice Chief of Staff, cited in "Status Report, Earth Satellite Vehicle," Committee on Guided Missiles, Research and Development Board, GM 13/15, January 15, 1948, 7.) No known equivalent satellite policy was offered by the Navy.

79. A Soviet historian has suggested that the decision not to proceed with construction of earth satellites at this time was due to "the vacillation and doubt regarding the feasibility of large-scale rockets in the U.S.A." (See I. N. Bubnov, "The Development of Large-Scale Rocket Designing in the USA, 1944-1948," *Transactions of the Ninth Scientific Conference of Graduate Students and Junior Scientific Associates of the Institute for the History of Natural Science and Technology*, Moscow, 1966, NASA TT F-540, August 1968, p. 19.) This contention is amply refuted by the record.

80. Modern astronautical theory (that is, as distinct from rocketry alone), quite noticeably preceded determined attempts to design and construct space vehicles, in contrast to development of the steam engine, for example, where much theory developed concurrently or followed empirical experimentation.

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LIQUID-HYDROGEN ROCKET ENGINE DEVELOPMENT
AT AEROJET, 1944-1950[+]

George H. Osborn, Robert Gordon, and Herman L. Coplen with George S. James (USA)[++]

I. INTRODUCTION

In the early 20th century, many early rocket pioneers favored liquid hydrogen; however, none of them used hydrogen as a rocket fuel because of its extreme physical properties and scarcity. Illustrative of the properties that make practical handling difficult is a boiling point of -426° at one atmosphere, and a density about one-seventh that of water. Interest in the methods and apparatus used in hydrogen gas liquefaction increased significantly in the mid-1940s when handling methods were developed to supply liquid hydrogen for the steadily increasing requirements of basic research. The authors had the good fortune to participate in one of the earliest programs in the United States to systematically investigate hydrogen-oxygen propellants for high-energy rocket engine application.

From late in 1944 to the cession of tests in August 1949, the hydrogen-oxygen programs at the Aerojet General Corporation, under the sponsorship of the Navy Bureau of Aeronautics, advanced these propellants from theoretical performance studies to practical sources of high specific impulse. Specifically, this work tested transpiration-cooled thrust chambers, investigated the concept of ablative-cooled thrust chambers, developed the first successful 1,000-lb-thrust gaseous-propellant rocket engine, conducted the first tests of the effect of jet overexpansion and separation on performance of rocket

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[+] Presented at the Fourth History Symposium of the International Academy of Astronautics, Constance, German Federal Republic, October 1970.

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thrust chambers, constructed and operated the first continuous 24-hour-operation hydrogen liquefaction plant specifically devoted to rocket engine use, conducted the first liquid-hydrogen tests of the coaxial injector, developed a 3,000-lb-thrust liquid-hydrogen thrust chamber, and tested the first pump to successfully produce high pressures in pumping liquid hydrogen, demonstrating that pumping liquid hydrogen in a turbo-rocket engine was perfectly feasible and could be accomplished with a single-stage centrifugal pump.

II. EARLY HYDROGEN-OXYGEN THRUST CHAMBER TESTS

In December 1944, Fritz Zwicky, then Director of Research for Aerojet, wrote a final report of research and development activities for the Bureau of Aeronautics, Navy Department, (Contract NOa(s)-3055). Zwicky surveyed the work being conducted at various institutions associated with the Navy program to produce chemicals commercially that would allow actual jet velocities between 9,000 feet/sec and 10,000 feet/sec.

These chemicals were mostly boron compounds of a nature not then commercially available.[1] The value of boron hydride compounds as a combined source of hydrogen and high chemical energy had been indicated early in 1944 by James M. Carter of Aerojet.[2] Subsequently, under Contract NOa(s)-5350, Donald L. Armstrong calculated that specific impulse of 311 at 600 psi chamber pressure was theoretically possible for an aluminum borohydride/water reaction.[3] In a separate report under the same contract Paul W. Webster calculated that the performance of hydrogen-oxygen as gaseous propellants would be substantially higher than that of the boron compounds.[4]

With this theoretical background, the first gaseous-hydrogen-oxygen thrust chamber tests were conducted at the Azusa proving grounds on October 15, 1945.[5] During the first test the uncooled thrust chamber burned out completely (injector, chamber, and nozzle) in 15 seconds. During the brief period of equilibruim, engineers measured a thrust of 45 lb at 375 psia chamber pressure with an estimated exhaust velocity of 8470 ft/sec. For the next test a specially designed water-cooled injector and a regulator water-cooled nitromethane type nozzle and chamber were used. Performance was 100 lb thrust at 295 psia chamber pressure with an estimated exhaust velocity of 7280 ft/sec. With the water-cooled thrust chamber an average heat flow density of 3-3 1/2 Btu/sec in^2 was measured, although the chamber eroded slightly in the region adjacent to the injector.[6]

By February 1946, the test facilities had been enlarged to allow the testing of thrust chambers of up to 500-lb thrust. Provision for water cooling the thrust chambers consisted of a 1,000-gallon water tank and a centrifugal pump which supplied 50 gallons per minute at 150 psi. Propellant was drawn from trailer trucks furnished by the National

Cylinder Gas Company. The oxygen trucks had a capacity of 33,000 cubic feet at standard conditions; the hydrogen trucks had a capacity of 28,000 cubic feet at standard conditions. With the given fuel capabilities it was possible to run a 500-lb-thrust chamber for five and one-half minutes before the hydrogen was depleted. A second series of tests with the 100-lb-thrust chamber was conducted during March 1946. The first test, using a mixing chamber injector burned out the injector after seven seconds; the second test with a modified injector was successful; and the third test ran for 60 seconds without damage. Starting, operating, and stopping the thrust chamber presented no unusual hazards. Starts and stops were very smooth and without any explosions.

> The motor was started by opening the propellant valves (an electric spark was on at all times) while holding the throttles closed. The throttles were then advanced maintaining a rich mixture (excess hydrogen) until the correct injection pressure was reached; both throttles could then be advanced or retarded together to vary the thrust while holding constant mixture ratio as in normal multi-engine airplane operation. However both throttles could be jockeyed separately in order to vary the mixture ratios. Control was positive and smooth throughout. The motor was stopped by closing the throttles to reduce fuel flow to a low value. The propellant valves were then closed.[7]

Development of 100- to 400-lb gaseous-hydrogen-oxygen thrust chambers continued for the Bureau of Aeronautics, under Task 6 of Contract NOa(s)-7968. Various techniques of cooling were explored including transpiration cooling (liquid and gas), convection cooling, radiation cooling, and heat capacity cooling. The most successful water-transpiration-cooled thrust chamber, shown in Figure 1, operated for 60 seconds without damage.

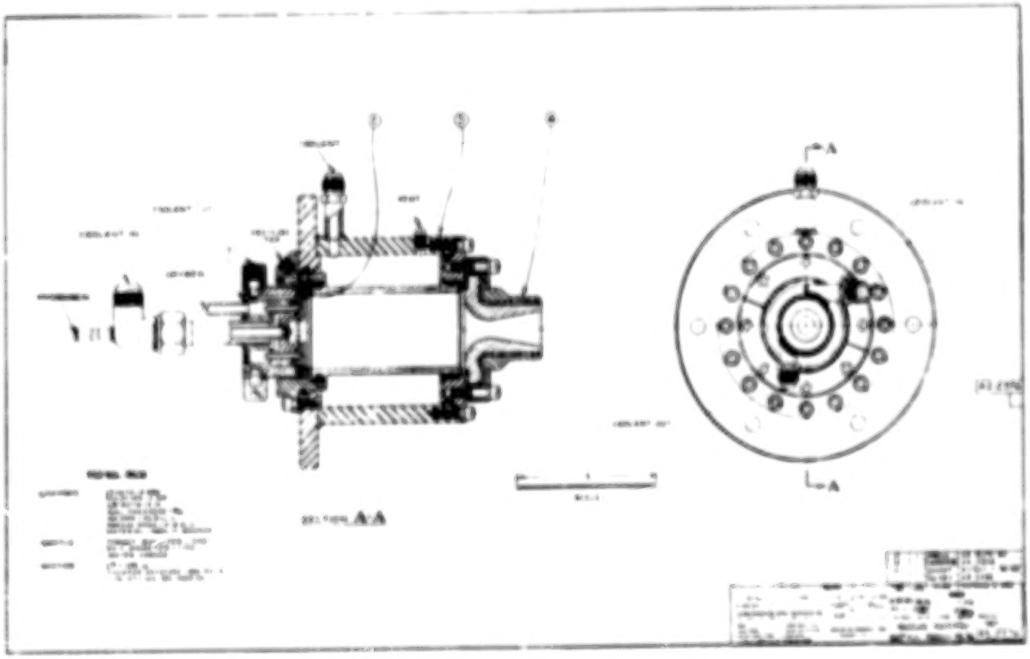

Fig. 1
Drawing of Porous Metal Thrust Chamber Assembly

The combustion chamber wall was made of porous bronze (Oilite). In subsequent tests the combustion chamber wall was cooled with gaseous hydrogen while the injector and nozzle were water cooled. An all convection-cooled thrust chamber of conventional design with a De Laval type nozzle was developed for comparison purposes. It successfully operated for 1 minute with high performance for such an early test (see Table I), and confirmed the performance of the hydrogen-oxygen propellants as predicted by Paul Webster.[8] This thrust chamber, shown in Figure 2, used externally circulated water as the cooling liquid to obtain basic heat transfer data for design of future motors and to develop a "work horse" motor that could be used to test experimental motor parts of any design.[9] Teflon chamber liners were tried as ablative liners to reduce the heat flow to the chamber. This they did effectively. However, the strength of Teflon when heated proved to be very low, and the material did not have sufficient structural strength to stay in the chamber for more than 15 seconds.

The rate of reaction for gaseous hydrogen-oxygen proved to be extremely rapid compared to liquid phase propellants. Consequently, a marked decrease in chamber volume (or L*) appeared possible. In considering designs of small L* thrust chambers, it seemed probable that a thrust chamber configuration consisting of a cylindrical chamber discharging into the divergent portion of a nozzle should give good performance. This configuration, called a "flared tube" type, appeared desirable from the nozzle cooling standpoint for small thrust chambers. It also appeared to be advantageous for large-thrust, high-altitude thrust chambers because of the rapid increase in nozzle size relative to the

TABLE I

PERFORMANCE OF 100-POUND-THRUST. CONVECTION-WATER-COOLED,

GASEOUS OXYGEN/GASEOUS HYDROGEN THRUST CHAMBER ASSEMBLY, 1946

Chamber Pressures	300 psia	500 psia
Thrust	100 lbs	100 lbs
H_2/O_2 Molar Ratio	3:1	3:1
I_{sp}	311 sec	336 sec
C	10,000 ft/sec	10,850 ft/sec
C*	7440 ft/sec	7970 ft/sec
C_F	1.341	1.360
L*	110 in.	188 in.

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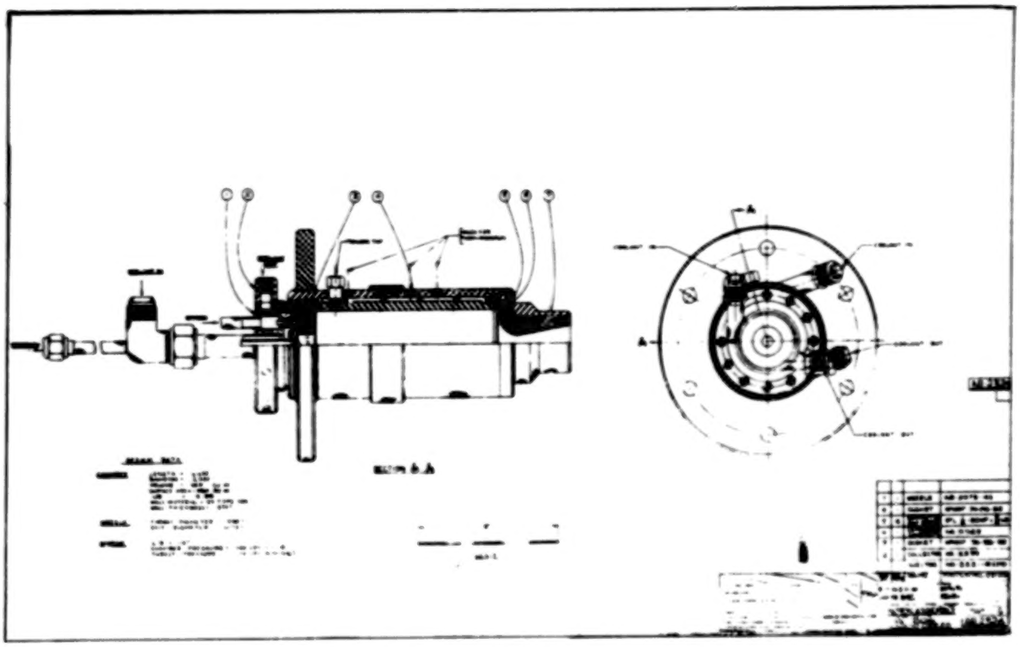


Fig. 2
Drawing of Convection-Cooled Oxygen/Hydrogen Thrust Chamber Assembly

combustion chamber. Consequently, engineers decided to make a flared tube thrust chamber with an L* of 10 from cast electrolytic copper. Results from the test of this thrust chamber led to the fabrication and testing of additional flared tube heat capacitance chambers with L*'s of 2 and 5. Maximum values of specific impulse from the four tests conducted during June 1946 were obtained over a range of L* values from 4.5 to 7 inches. For L* values of 2.0 inches or greater, Isp was not less than 300 seconds. These initial tests indicated that the flared tube thrust chamber configuration was efficient and could result in savings in weight and could simplify thrust chamber design.

As a result of the above tests, two water-transpiration-cooled 300-lb-thrust chambers were constructed from Oilite (Figure 3). However, the coolant flow rates were found to be very low and the porosity of the particular pieces of Oilite much less than that of previous pieces. In tests, the thrust chamber suffered erosion due to the lack of adequate cooling. The final report concluded that the operation of thrust chambers at exhaust velocities above 10,000 ft/sec (310 sec Isp) had become commonplace using gaseous hydrogen and gaseous oxygen. The report recommended that hydrogen, especially liquified hydrogen, offered real and immediate benefits for long-range, large-scale rocket propulsion.[10]

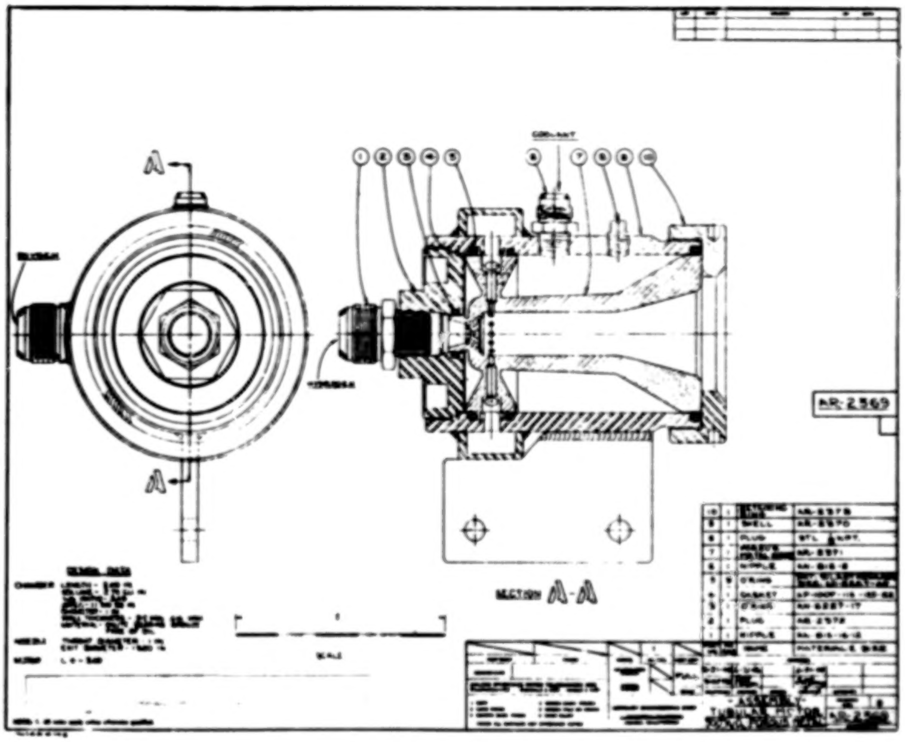


Fig. 3
Drawing of Oilite Flared Tube Thrust Chamber Assembly

III. 300,000-LB-THRUST HYDROGEN-OXYGEN ENGINE DESIGN STUDY

The requirements of Navy Contract NOa(s)-8496 issued in July 1946 called for the design study of a 300,000-lb-thrust rocket engine using liquid hydrogen and oxygen as propellants, and the development of a gaseous-hydrogen-oxygen rocket thrust chamber of 1,000-lb thrust capable of three minutes operation at 300 seconds Isp. The design study would provide a liquid-hydrogen-oxygen rocket engine suitable for use in a high-altitude test vehicle, such as the single-stage satellite vehicle, under study by the Glenn L. Martin Company under Contract NOa(s)-8376[11] and North American Aviation under Contract NOa(s)-8349.[12]

The target specifications included a rated thrust of 300,000 pounds in a vacuum, a specific impulse of 310 seconds at sea level and 425 seconds in a vacuum, a weight less than 4,000 pounds, and a duration greater than 300 seconds.[13] Tables II and III present additional details. Activities devoted to a 1,000-lb-thrust gaseous engine supplied experimental data for the design study, as discussed in Section IV. Various thrust chamber configurations were investigated with special reference to the variation of performance and pressure distribution in tubular and flared tube thrust chambers.

General observations were made in an effort to determine the thermodynamic behavior of the propellant gases. Transpiration cooling was investigated with special reference to the development of sources of supply of the various types of porous materials and to the determination of the flow of coolant required for the protection of the thrust chamber walls.

TABLE II

SUMMARY OF TARGET SPECIFICATIONS FOR 300,000-POUND-THRUST LIQUID-OXYGEN/LIQUID-HYDROGEN ROCKET ENGINE

ENGINE DATA:

F = 300,000 lbs

I_{sp} = 425 seconds

P_c = 500 psia

w = 689 lbs propellant per sec

M.R. = 3 mols H_2 to 1 mole O_2 (6 lbs H_2 + 32 lbs O_2/38 lbs mixture)

D_f = 4.40 lbs/ft^3 (density)

D_o = 71.2 lbs/ft^3 (density)

P_p = 750 psia (pump discharge pressure)

VEHICLE DATA:

89,000 lbs propellant

14,100 lbs hydrogen

74,900 lbs oxygen

3,220 ft^3 hydrogen

1,050 ft^3 oxygen

40-psia gas pressure over propellants in tanks

15-psia vapor pressure of propellants in tanks

TABLE III

SUMMARY OF PUMP DESIGN DATA FOR 300,000-POUND-THRUST LIQUID-OXYGEN/LIQUID-HYDROGEN ROCKET ENGINE

	LH_2	LO_2
Capacity (Q gpm)	11,100	3,660
Propellant Vapor Pressure Heads (H_v ft) (15 psia)	490	30.3
Suction Head (H_s ft)	1,353	117
Suction Head Above Vapor Pressure (H_{sv} ft)	863	86.7
Discharge Head (H_d ft)	24,550	1,517
Total Head (H ft)	23,200	1,400
Cavitation Constant (o)	.0372	.0621
Shaft Speed (N rpm)	11,000	11,000
No. Impeller per Stage	1	6
No. Stages	4	1
Impeller Diameter (d in.)	13	6.25
Specific Speed (N_s rpm)	1,860	1,180
Water Horsepower ($H.P._w$)	4,610	1,480
Efficiency (%) (Estimated)	75	75
Shaft Horsepower ($H.P._s$)	6,150	1,970
Total Shaft Horsepower Required	8120	

During the 300,000-lb-thrust chamber design study the following assumptions were made: (a) Propellants would be injected as liquid hydrogen and liquid oxygen, (b) the performance of the propellants calculated on a basis of non-dissociation of the combustion products, (c) two percent of the total propellant flow would be diverted for the turbine, and (d) the hydrogen required to cool the motor would be available from the extra mole of hydrogen in the 3:1 molar ratio.[14] The requirements led to the choice of a transpiration cooled flared tube thrust chamber with an extremely high expansion ratio.

It was believed that the flared tube thrust chamber would help keep the weight of the propulsion system down, while the high expansion ratio and the propellants chosen would give the required performance. The large expansion ratio decreased the relative size of the combustion chamber and resulted in a thrust chamber which appeared to be almost entirely an expanding section of a normal de Laval nozzle.

Press and furnace capacity limited the available size of porous metal sheets. Consequently, a metal spray technique under development by J. Wulff of the Massachusetts Institute of Technology was proposed for construction of the 33-percent porous stainless steel combustion chamber inner liner. The material chosen for the outer shell of the thrust chamber was Stainless W, an experimental alloy produced by the United States Steel Company.[15]

Pump capacities, determined by the thrust and specific impulse of the thrust chamber, the mixture ratio and the propellant densities, were 11,100 gallons/minute of liquid hydrogen and 3,660 gallons/minute of liquid oxygen. Because pump vapor pressure head at the entrance of the pump affects pump cavitation, efforts were made to minimize this factor. It was assumed that the propellant would be carried in special tank trucks under atmospheric pressure until being delivered to the test vehicle propellant tanks. It was also assumed that the tanks, pipes, valves, and pumps would be cooled by evaporation of a small quantity of propellant so that when the bulk of the propellant supply had been delivered to the test vehicle propellant tanks it would have practically the same temperature and hence the same initial vapor pressure as it had in the tank trucks. This would result in a hydrogen vapor pressure head of 490 feet of liquid hydrogen and an oxygen vapor pressure head of 30.3 feet of liquid oxygen (see Table III above).

The design of the gas turbine was undertaken with the realization that only a basic design could be formulated without extensive experience in the use of hydrogen and oxygen as gas turbine propellants. It was decided that the propellant combination used for the main thrust chamber would also be used for the turbine because of its high specific impulse, and because it helped keep the test vehicle mass ratio as high as possible. The turbine design data is summarized in Table IV.[16] The final report on the engine design study, issued on 31 March 1947,[17] concluded that a 300,000-lb-thrust engine was entirely feasible. Though many detail problems remained there were no fundamental reasons why such a propulsion system could not be built to propel a single stage satellite vehicle as shown in Figure 4.

The 300,000-lb-thrust chamber was designed to give the performance of 425 seconds specific impulse as specified. A chamber pressure of 500 psia was assumed. Calculations indicate that the total power plant weight would remain essentially constant between chamber pressures of 300 and 500 psia, so the latter figure was chosen. Performance would be affected by the mixture ratio of the propellants. Increasing the hydrogen

TABLE IV

SUMMARY OF TURBINE DESIGN DATA FOR 300,000-POUND-THRUST

LIQUID-OXYGEN/LIQUID-HYDROGEN ROCKET ENGINE

Pump Horsepower ($H.P._p$)	8,120
Accessory Horsepower ($H.P._{acc}$)	80
Turbine Horsepower ($H.P._t$)	8,200
Turbine Fuel	Hydrogen
Turbine Oxidizer	Oxygen
Bucket Velocity (U ft/sec)	1,200
Turbine Wheel Working Stress (S_t lbs/in^2)	30,000
Bucket Temperature (T_b)	1,500
Chamber Temperature (T_c °F) (T_c °F)	1,580 1,130
Mixture Ratio (M.R. mols hydrogen: mols oxygen)	14:1
Chamber Pressure (P_c lbs/in.^{2})	500
Discharge Pressure (P_e lbs/in.^{2})	18
Discharge Temperature (T_e °K)	460
Enthalpy of Products of Combustion in Chamber (h_c Kcal/gm)	1.89
Ratio of Specific Heats of Products of Combustion (γ)	1.37
Enthalpy of Products of Combustion at Exit (h_e Kcal/gm)	0.74
Change in Enthalpy of Products of Combustion (Δh Kcal/gm)	1.15
Discharge Velocity (C ft/sec)	10,180
Heat and Turbulence Loss Factor	0.90
Absolute Velocity of Gases Entering Buckets (V_1 ft/sec)	9,650

TABLE IV (Continued)

SUMMARY OF TURBINE DESIGN DATA FOR 300,000-POUND-THRUST LIQUID-OXYGEN/LIQUID-HYDROGEN ROCKET ENGINE

Absolute Angle of Gases Entering Buckets (α_1, deg)	26.5
Relative Velocity of Gases Entering Buckets (V_2 ft/sec)	8,600
Relative Angle of Gases Entering Buckets (B_1 deg)	30
Bucket Loss Coefficient (Y)	0.645
Relative Velocity of Gases Leaving Buckets (V_3 ft/sec)	5,550
Relative Angle of Gases Leaving Buckets (B_2 deg)	30
Absolute Velocity of Gases Leaving Buckets (V_4 ft/sec)	4,560
Vectorial Difference in Tangential Components of Absolute Velocities of Gases Entering and Leaving Buckets (ΔV_t ft/sec)	11,000
Mechanical Efficiency of Turbine (Estimated)	0.94
Propellant Consumption (W lbs propellant/sec)	11.7
(W_f lbs hydrogen/sec)	5.46
(W_o lbs oxygen/sec)	6.24

above stoichiometric would reduce the combustion temperature and increase the performance. However increasing the hydrogen content would reduce the density impulse.[18] Since the optimum performance occurred at about 3:1, this mixture ratio was chosen. The calculated performance at sea level was 330 seconds specific impulse at 223,600-lb thrust. As designed, this thrust chamber had an exit diameter of 13 1/2 ft, a chamber diameter of 2 ft, and a length of 22 1/2 ft. It would be hydrogen transpiration cooled, with separate coolant control to 28 compartments. The permeable inner liner consisted

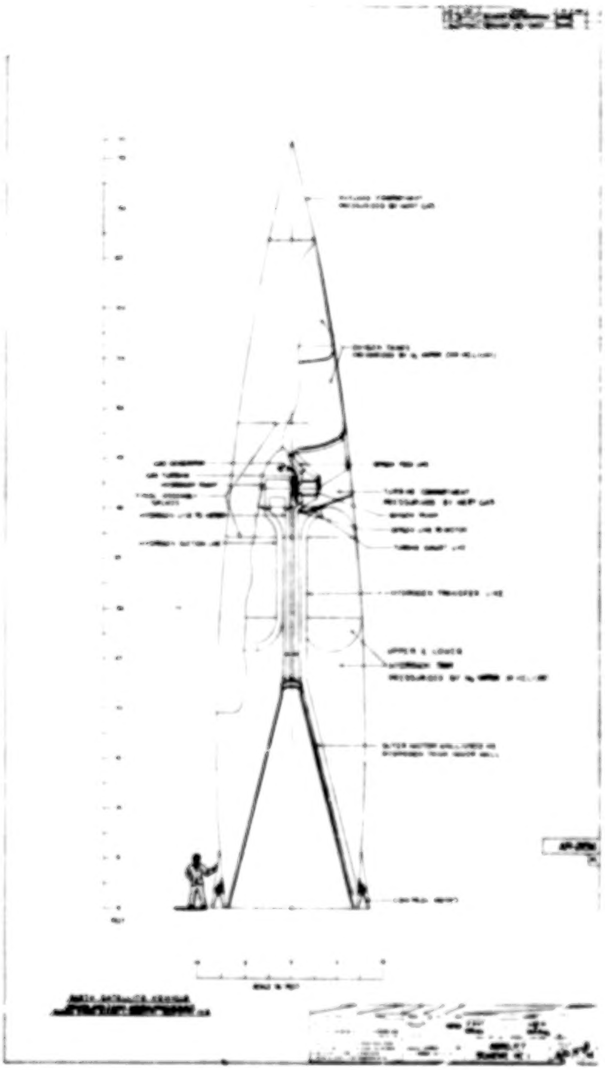


Fig. 4
Drawing of Earth Satellite Vehicle (Glenn L. Martin Scheme No. 5)

of 33% porous stainless steel, 1/8 to 3/16 inch thick. The outer motor wall, circular ribs, and longerons were of Stainless W. The calculated total thrust chamber weight was 2678 lb.

In the pump design studies the factors affecting shaft speed, number of impellers per stage, number of stages and impeller diameter were analyzed. Pump layouts were prepared and weight calculated. In the turbine design study the factors affecting propellant consumption and turbine wheel stresses were analyzed. A turbine layout was prepared and the weight calculated. As a result of these studies it was concluded that a turbopump of the following description (Figure 5), would be suitable for pressurizing the liquid-hydrogen and liquid-oxygen propellants for a 300,000-lb-thrust high-altitude rocket engine:

Weight of turbopump	850 pounds
Length of turbopump	70 inches
Maximum diameter of turbopump	30 1/4 inches

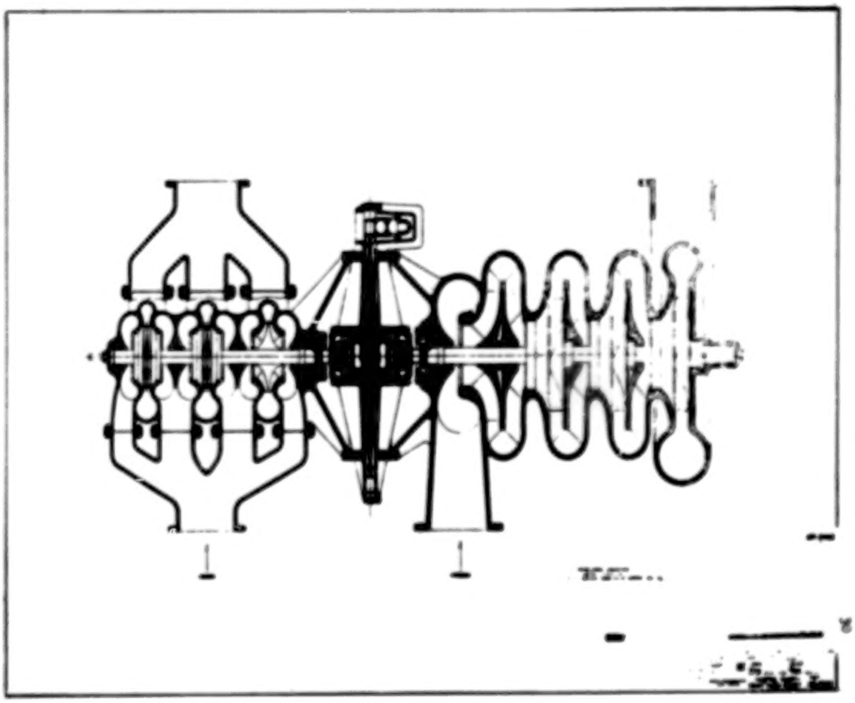


Fig. 5
Drawing of Turbopump Assembly, 300,000-Pound-Thrust Engine

Four small engines mounted parallel to the center line appeared the best method of controlling roll, pitch, yaw, and for changing the trajectory of the satellite vehicle. These engines would operate continuously and would change the direction of their thrust by pivoting their thrust chambers. The size of these control engines could not be established without a final determination on the amount of turning moment required to control the vehicle. Turning the small thrust chambers with servo mechanisms presented no special problems.

A schematic diagram showing the major elements of the main engine controls, the small engines, and the small engine controls is presented in Figure 6. The valves and

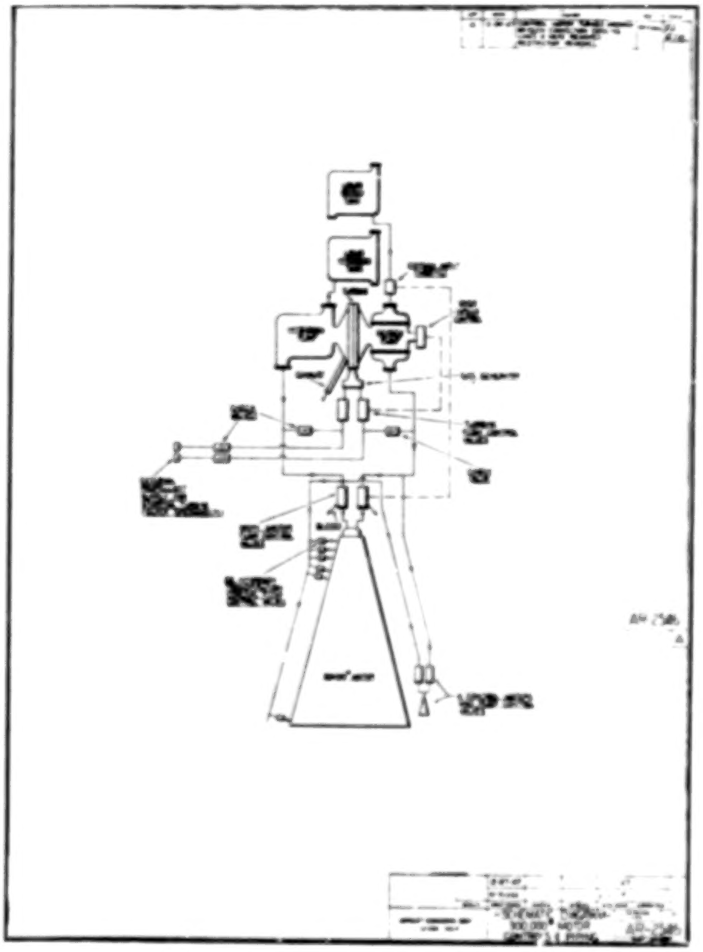


Fig. 6
Schematic Diagram, 300,000-Pound-Thrust Engine, Controls

plumbing, because of the very large size and low temperature, offered problems well beyond any available equipment. The weight of these elements including the necessary insulation was estimated at 355 pounds.

The dry weight of the engine, not including the control engines, was as follows:

Thrust Chamber	2678 pounds
Turbopump unit	850 pounds
Valves and plumbing	355 pounds
	3883 pounds

The total weight given above was within the target weight. Aerojet recommended that a development program be instigated in order to find solutions to the fabrication and propellant handling problems brought out by this design study.

IV. 350-LB-AND 1000-LB-THRUST GASEOUS-HYDROGEN-OXYGEN THRUST CHAMBERS

During the work to produce a 1,000-lb gaseous engine, engineers developed a 350-lb-thrust, 5 1/2-inch L*, flared tube, water-convection-cooled, thrust chamber that was operated undamaged for periods of more than 1 minute with gaseous hydrogen and gaseous oxygen as propellants.[19] This thrust chamber (Figure 7), operating with a 3 H_2/O_2 molar mixture ratio at 300-psia chamber pressure, delivered 330 seconds specific impulse (95% of theoretical Isp).

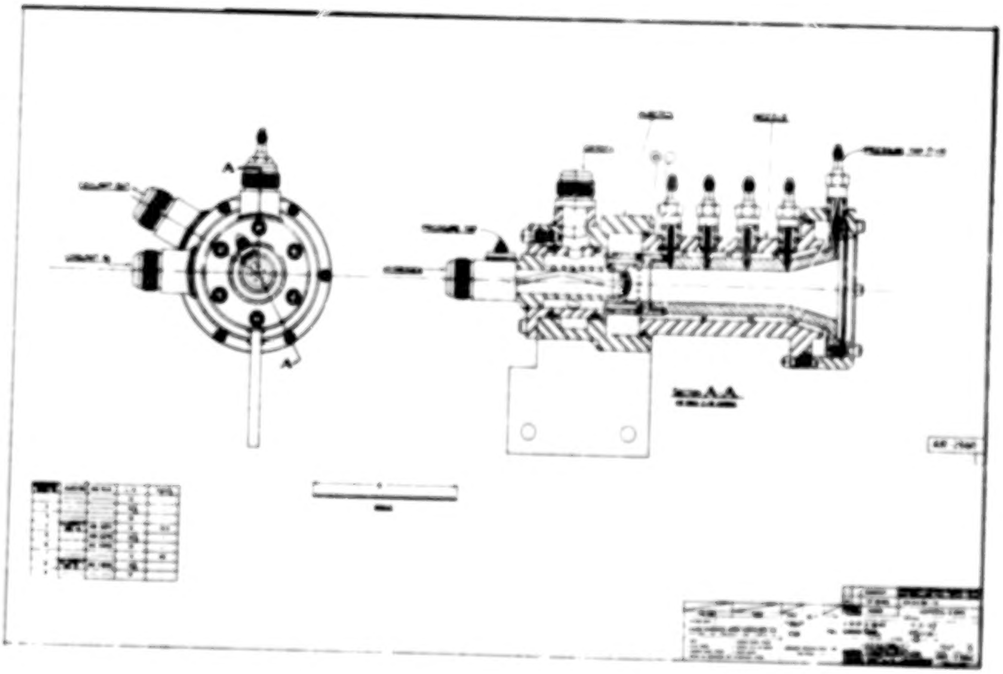


Fig. 7
Drawing of 400-Pound Liquid-Cooled Thrust Chamber Test Assembly

On June 26, 1947, a 1000-lb-thrust chamber which completely fulfilled the aims and specifications of Contract NOa(s)-8496 was successfully tested. The specification test was made on the thrust chamber (Figure 8) with a 19 1/2 percent coolant water flow. The configuration was a modified flared tube, with the nozzle portion of the motor water transpiration cooled, using porous nickel as the liner material. The injector was convection cooled with the water subsequently used for transpiration cooling. The test run was terminated by the operator after 190 seconds elapsed time, of which 183 seconds were at full performance.[20] The minimum performance maintained for the 183 seconds period appears below.

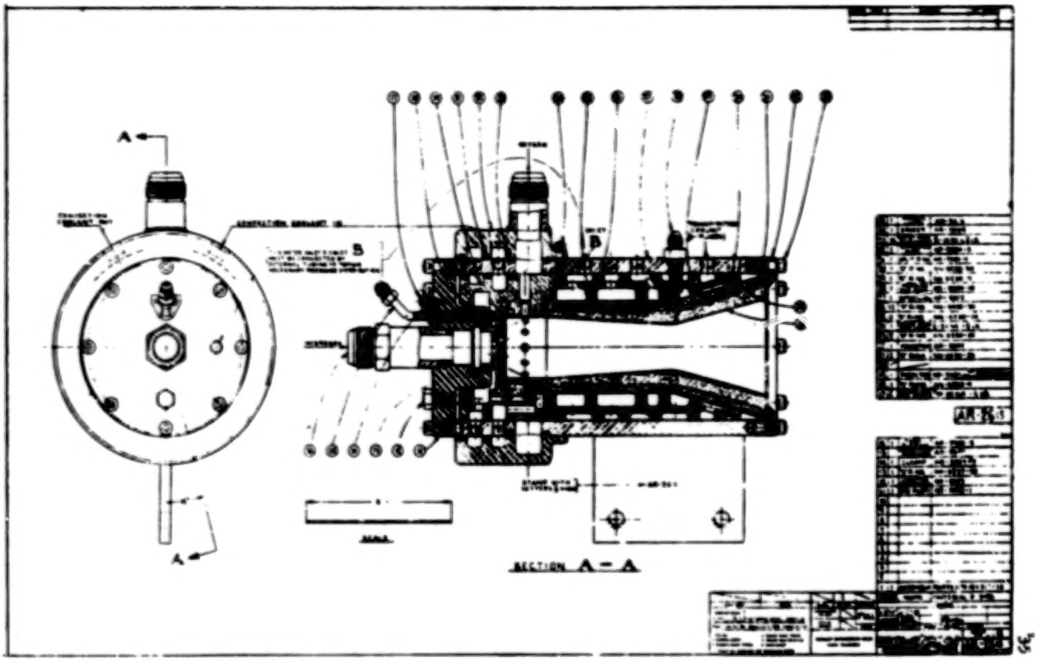


Fig. 8
Drawing of 1000-Pound-Thrust, Flared-Tube, Hybrid-Cooled Thrust Chamber

Thrust = 1230 lb
Isp = 309 seconds
P_c = 500 psia
H_2/O_2 = 4:1 molar ratio
Coolant = 19 1/2%

These experimental thrust chambers were designed on the same principals used in the design of the motor of the 300,000-lb-thrust rocket engine; in general, they confirmed the design data for the large thrust chamber.

V. LARGE-SCALE PRODUCTION AND HANDLING OF LIQUID HYDROGEN FOR THE XLR16-AJ-2 ENGINE AND PTV-N-3 VEHICLE REQUIREMENTS

During July 1947, Contract NOa(s)-8496 was amended to authorize the development of a liquid oxygen-liquid hydrogen engine, the XLR16-AJ-2, suitable for use in a small scale version of an earth satellite vehicle, the PTV-N-3, under study by the Glenn L. Martin Company.

The initial target specifications of the engine were:[21]

Thrust	2000-3000 lb
Duration	60 seconds
Isp	303 seconds minimum
Weight	75 lb maximum
Engine Inlet Pressure	35 psia maximum

This section of the paper describes the development of the Aerojet liquid hydrogen plant which supplied the original 4.7-pound (30 liter)-per-hour requirements, and, subsequently in August 1948, the requirements of 12 pounds (76 liters) per hour when the engine thrust was increased to 3000 lb, nominal, and its duration of burning extended to 180 seconds.[22] The revised XLR16-AJ-1 engine and PTV-N-3 vehicle specifications are shown in Tables V and VI. In 1947, the requirement for liquid hydrogen for the Aerojet contract had been initially estimated at 3000 to 6000 pounds depending on the cost. In addition, the Jet Propulsion Laboratory at the California Institute of Technology required from 1300 to 2000 pounds for an Army Ordinance sponsored program.[23] This combined demand could be met by a 25 liters/hour hydrogen liquefier similar to the installation at the Ohio State Cryogenic Laboratory, designed by Herrick L. Johnston.[24]

TABLE V

SUMMARY OF SPECIFICATIONS FOR THE XLR16-AJ-2 ROCKET ENGINE

Propellants:	Liquid hydrogen and liquid oxygen.
Motor:	Single cylinder, fully gimbaled. Maximum deflection ±15° with approximately 5 cycles/second response. Servo mechanism not a portion of the motor.
Mixture Ratio:	4-1/2:1 hydrogen to oxygen molar ratio, overall. (Note this change from 4:1 is made to facilitate motor cooling and will require Bureau of Aeronautics approval).
Specific Impulse:	303 sec minimum.
Thrust:	3000 lb nominal at sea level. (Exact definition is 10 lb/sec of propellant consumption at above mixture ratio.)
Duration:	3 minutes nominal.
Propellant Pressurization:	Turbopump, same propellants.
Weight Breakdown:	Motor, pumps, and valves . . . . 85 lb Gimbal ring . . . . 18 lb
Suction Pressure (NPSH):	35 psia nominal, subject to experimental verification.

TABLE V (Continued)

SUMMARY OF SPECIFICATIONS FOR THE XLR16-AJ-2 ROCKET ENGINE

Dimensions:	To fit into a truncated cone approximately 2 ft dia. at motor end, 3 ft diameter at turbopump end (this diameter to be considered fixed) and approximately 4 1/2 ft long.
Electric Power:	24 volts dc from external source.

TABLE VI

SUMMARY OF SPECIFICATIONS FOR THE PTV-N-3 PROPULSIVE TEST VEHICLE

Propellants:	Liquid hydrogen and liquid oxygen.
Weight:	2000 - 2500 pounds gross.
Payload:	95-lb telemetering gear.
Mass Ratio:	0.65 - 0.7 propellant weight/gross weight nominal.
Weight:	1300 - 1750 pounds.
Structure:	Pressurized thin-skin integral-tank construction. (Stainless Steel estimated at .020-inch thick.)
Pressurization:	Evaporated propellants at 35 psia nominal.
Power Plant:	Aerojet XLR16-AJ-2 rated at 3000-lb thrust nominal.
Length:	25 - 29 ft depending on gross weight.
Diameter:	3 ft maximum.
Shape:	Ogive nose (65-ft radius) 13 ft long. Cylindrical body (3-ft dia.) 11 ft. Boat Tail Motor Corp. (1.96 ft minimum) 4 1/2 ft.
Control:	Fully gimbaled motor for pitch and yaw control. Turbine exhaust for roll control. Controls powered by hydraulic pressure.
Filling:	External support for structure allowing propellants to be held at 1 atm pressure (or subcooled) until immediately prior to take-off. Oxygen filling connections in space between tanks, hydrogen filling in motor compartment or in space between tanks.
Insulation:	a) Internal fuzz type. b) External drop-away blanket, helium filled.
Motor Compartment:	Helium filled prior to firing.
Electrical Supply:	24 volts dc.

Commercial producers were contacted but they either believed large-scale production of liquid hydrogen to be unfeasible, or quoted prices considerably higher than would be incurred if Aerojet constructed and operated a plant at Azusa. Additional checking on the cost of an Aerojet-built plant indicated that the total cost to build and operate it would be $100,000. In view of the considerable difference between the commercial and the Aerojet estimate in both cost and convenience, a request was made to the Bureau of Aeronautics for permission to erect and operate the plant at Azusa. Aerojet received verbal approval in September 1947, and action was initiated to obtain the consulting services of H. L. Johnston of Ohio State University.

Dr. Johnston spent October 13-16 at Aerojet in conferences on the detailed design of a plant utilizing commercial gaseous hydrogen and liquid nitrogen as a precoolant instead of liquid air. He returned to Ohio State to prepare drawings of the special units required for the liquefaction cycle and to supply certain design specifications to assist Aerojet's design of the more conventional special units. Nearly all commercial component parts were located and specified for purchase during October. Preliminary plant layouts established building requirements.

Following the receipt of formal authorization on December 16, 1947, purchase orders were placed for all major commercial units required except the freon refrigeration system for which complete specifications were not yet available.[25] Construction of the Aerojet building to house the liquefier also began in December. Transport of the liquid hydrogen pressure vessels to and from the liquefier building was planned to be done by a heavy duty lift truck.

The Aerojet cycle was a modified form of the cycle used at the Ohio State University Cryogenic Laboratory.[26] The flow of hydrogen from gas to liquid is shown in Figure 9. The Aerojet plant utilized the liquid nitrogen precooled Joule-Thomson (Onnes) cycle because suitable heat-exchanger cryostat designs were available for a plant of this size. The use of an established cycle was necessary because of the urgent need for a propellant in the test programs. Major modifications made in the purification portions of the cycle included:

1) Provision to utilize a commercial gaseous hydrogen supply,
2) Continuous catalytic removal of the oxygen impurity in the commercial gaseous hydrogen supply,
3) Adsorption purification at liquid nitrogen temperatures to remove any remaining gaseous nitrogen and oxygen from the hydrogen upstream of the hydrogen expansion valve,
4) Utilization of a commercial liquid nitrogen supply for the liquefier precooler interchanger, and
5) Use of parallel purification units where necessary to allow continuous operation over extended periods.

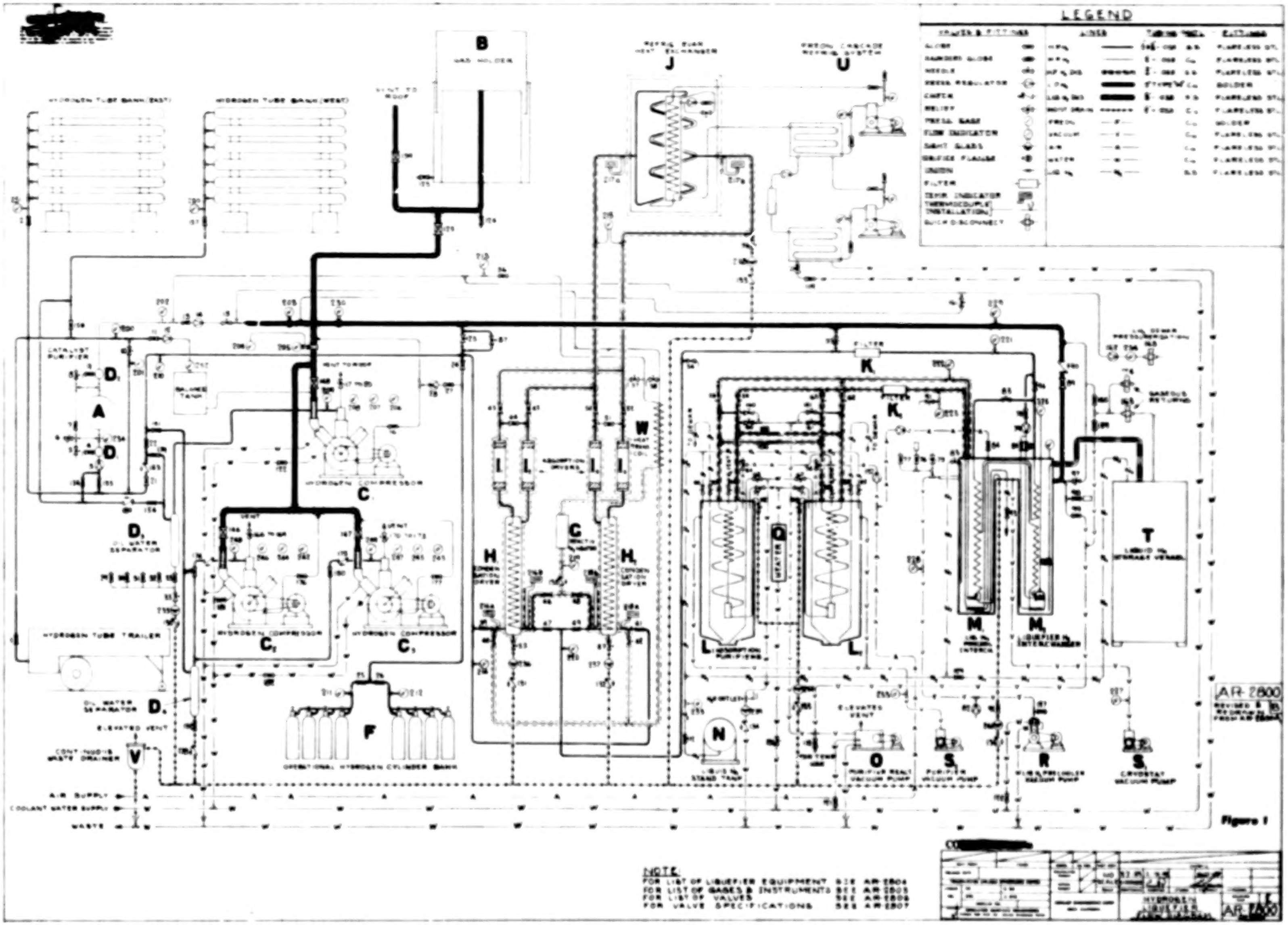

[redacted]
LEGEND
HYDROGEN TUBE BANK (EAST)
HYDROGEN TUBE BANK (WEST)
VENT TO ROOF
B
GAS HOLDER
REFRIG. EVAP. HEAT EXCHANGER
J
FREON CASCADE REFRIG. SYSTEM
U
CATALYST PURIFIER
A
D₁
D₂
BALANCE TANK
HYDROGEN COMPRESSOR
C
D₃
OIL WATER SEPARATOR
HYDROGEN COMPRESSOR
C₁
HYDROGEN COMPRESSOR
C₂
HYDROGEN TUBE TRAILER
OIL WATER SEPARATOR
D₄
ELEVATED VENT
CONT. NUDDIS WASTE DRAINER
V
AIR SUPPLY
COOLANT WATER SUPPLY
WASTE
OPERATIONAL HYDROGEN CYLINDER BANK
F
I
I₁
I₂
I₃
ADSORPTION DRIERS
H
H₁
G
W
K
K₁
FILTER
Q
HEATER
L
L₁
ADSORPTION PURIFIERS
M
M₁
LIQUEFIER INTERCHANGER
T
LIQUID H₂ STORAGE VESSEL
GASEOUS RETURNS
N
LIQUID H₂ STAND TRAP
ELEVATED VENT
O
PURIFIER REGEN. VACUUM PUMP
S
PURIFIER VACUUM PUMP
R
H₂ REFRIGERATOR VACUUM PUMP
S₁
CRYOSTAT VACUUM PUMP
AR-2800
Figure I
CO[redacted]
NOTE:
FOR LIST OF LIQUEFIER EQUIPMENT SEE AR-2804
FOR LIST OF GASES & INSTRUMENTS SEE AR-2805
FOR LIST OF VALVES SEE AR-2806
FOR VALVE SPECIFICATIONS SEE AR-2807
HYDROGEN LIQUEFIER
AR-2800

Fig. 9
Hydrogen Liquefier Flow Diagram

On May 21, 1948, a ten-hour operating test was made on the hydrogen liquefier cryostat assembly which had been completed and installed in the liquefaction plant system at the Ohio State University Cryogenic Laboratory. The test was supervised by H. L. Johnston. H. L. Coplen observed and assisted in the operation of the unit. The performance of the unit was very satisfactory, especially in its economy of liquid air for precooling.[27] The liquefier cryostat assembly was subsequently shipped and installed in the Azusa plant.

The first production of liquid hydrogen occurred on September 3, 1948, and furnished about 12 liters of liquid hydrogen. Subsequently, a number of improvements, adjustments, and repairs were made to the system to correct discovered faults. The plant was again operated on September 21, 22, and 23. On September 22, approximately 120 liters were produced of which a net of 75 liters (11.75 lbs) was supplied to the Jet Propulsion Laboratory of the California Institute of Technology for their rocket test program.

As originally designed, the plant had a production capacity of 4.7 pounds (30 liters) per hour and was so operated from September 1948 to March 1949. Because of the increase in propellant requirements resulting from the 3000-lb-thrust chamber tests, the plant shown in Figures 10, 11, and 12 was improved to increase the capacity to approximately 12 pounds (76 liters) per hour, and it was operated on a three-shift basis

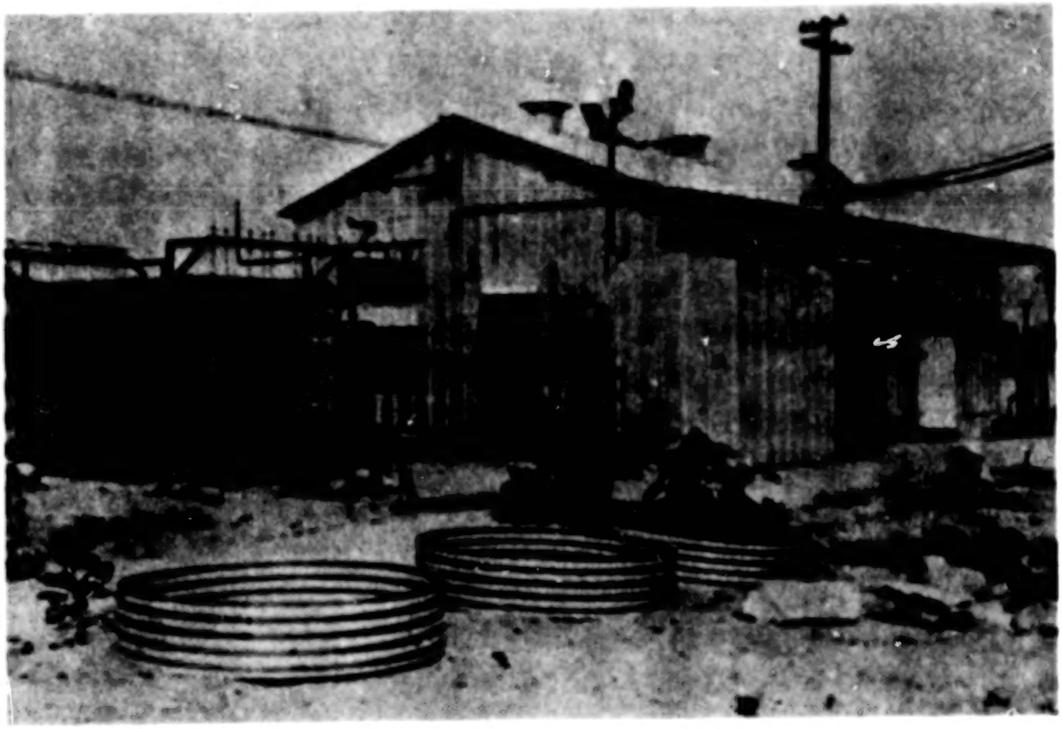
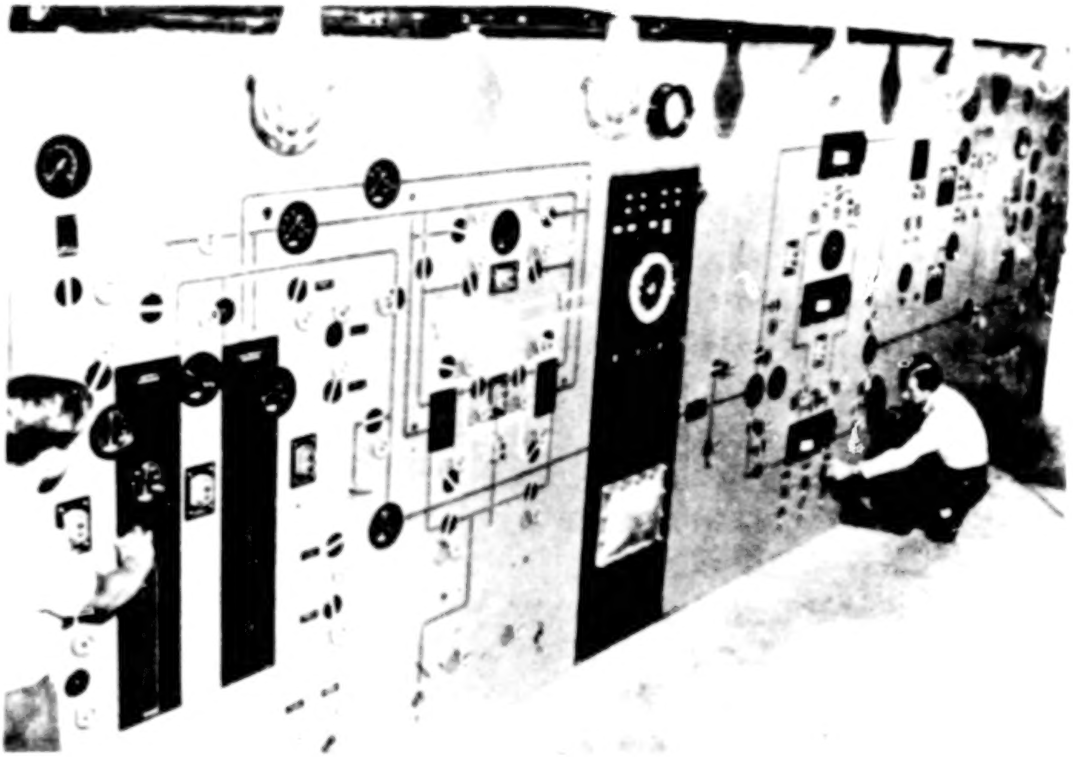


Fig. 10
Aerojet Liquid-Hydrogen Plant



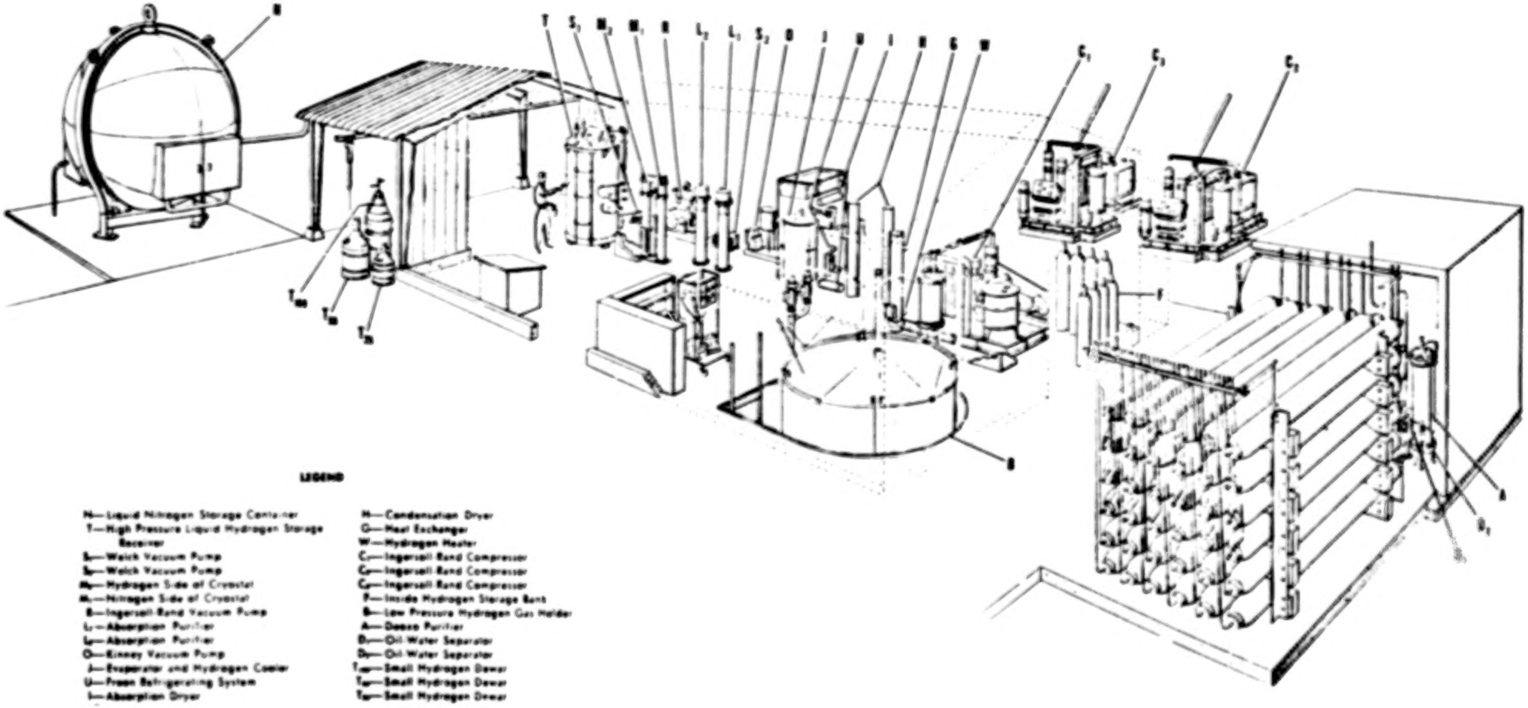

N
T
S_1
M_2
M_1
R
L_2
L_1
S_2
O
J
U
I
H
G
W
C_1
C_2
C_3
F
B
A
D_2
D_1
T
T
T
LEGEND
N—Liquid Nitrogen Storage Container
T—High Pressure Liquid Hydrogen Storage Receiver
S_1—Welch Vacuum Pump
S_2—Welch Vacuum Pump
M_2—Hydrogen Side of Cryostat
M_1—Nitrogen Side of Cryostat
R—Ingersoll-Rand Vacuum Pump
L_1—Absorption Purifier
L_2—Absorption Purifier
O—Kinney Vacuum Pump
J—Evaporator and Hydrogen Cooler
U—Freon Refrigerating System
I—Absorption Dryer
H—Condensation Dryer
G—Heat Exchanger
W—Hydrogen Heater
C_1—Ingersoll Rand Compressor
C_2—Ingersoll Rand Compressor
C_3—Ingersoll Rand Compressor
F—Inside Hydrogen Storage Bank
B—Low Pressure Hydrogen Gas Holder
A—Deoxo Purifier
D_1—Oil-Water Separator
D_2—Oil-Water Separator
T—Small Hydrogen Dewar
T—Small Hydrogen Dewar
T—Small Hydrogen Dewar

Fig. 12
Hydrogen Liquefier Plant Layout

impulse and propellant density on vehicle performance involved a vehicle with the following requirements:

1) 300-mile-altitude satellite orbit
2) 1000-lb payload
3) Single-stage vehicle[+]
4) Sea-level, non-boosted takeoff

After some consideration of the complexities of the actual trajectory required to reach a stable orbit, engineers sought the vertical altitude which would result from imparting orbital energy to the vehicle. In determining the energy of a satellite vehicle in a stable orbit the variation of gravity with altitude was taken into account. The energy of a satellite in an orbit at 300-mile altitude was assumed to be 10.12×10^6 ft-lb/lb (sea-level ft-lb of energy). Vertical trajectory calculations were performed by a Runge-Kutts stepwise integration of the basic equation of motion, using a perturbation technique including the effect of drag. The altitude and kinetic energy were added at the end of burning to obtain an equivalent total energy expressed as an energy altitude in feet. The results were presented as a series of curves giving this energy altitude as a function of the mass ratio for different sets of values of the standard specific impulse, vehicle density loading, and total initial acceleration.

These calculations served to show the effect of specific impulse on vehicle performance. The effect of propellant density is concealed by the fact that vehicle density loading and mass ratio, both dependent on density, are not expressed as direct functions of density. The gross vehicle weight was determined as the sum of all the vehicle components divided into suitable groups, and expressed as functions of the pertinent vehicle and propellant properties. It was found possible to evaluate the motor, pumping plant, piping, valves, and controls by means of available data on existing vehicles and by detailed design studies.

The structural weight did not lend itself to the above-noted method of analysis. In order to achieve a mass ratio adequate for satellite performance, it was necessary to use unconventional fabrication techniques. The best method had been proposed by the Glenn L. Martin Company, Baltimore, Maryland, and North American Aviation Inc., Inglewood, California. It comprised the use of a highly stressed thin skin in a pressurized structure with integral propellant tanks. The pressure placed all the skin material under tension sufficient to prevent any compressive loads. The ribs and stringers required to carry compression loads in conventional structures could be omitted in this way, and the skin could be highly stressed.[29]

[+] See R. Cargill Hall, "Earth Satellites, A First Look By The United States Navy," in this volume - Ed.

The sum of all the vehicle components, propellant and payload weights was then used as the gross weight. The results were prepared in a series of curves in which mass ratio and density loading were shown as functions of gross weight for a given set of values of propellant density, total takeoff acceleration, and specific impulse at standard conditions. Almost none of the vehicle components were weight dependent on specific impulse. The assumptions and methods used in this work had been evaluated by H. S. Tsien of the Massachusetts Institute of Technology, a consultant for this program.[30] These calculations served to show the relation between propellant density and mass ratio, and between propellant density and weight loading for a number of vehicles suitable for satellite or extreme long-range performance.

The results shown in Figure 13 indicated that specific impulse is much more important than propellant density. This result contradicted the findings of many reputable groups and individuals in the field of rocketry at that time. The author, Robert Gordon, cautioned that the results of the report should not be indiscriminately applied to all rockets under all conditions. Such a practice would be as indefensible as was the practice of applying A-4 (V-2) data and performance to every new application no matter how far removed from the A-4.

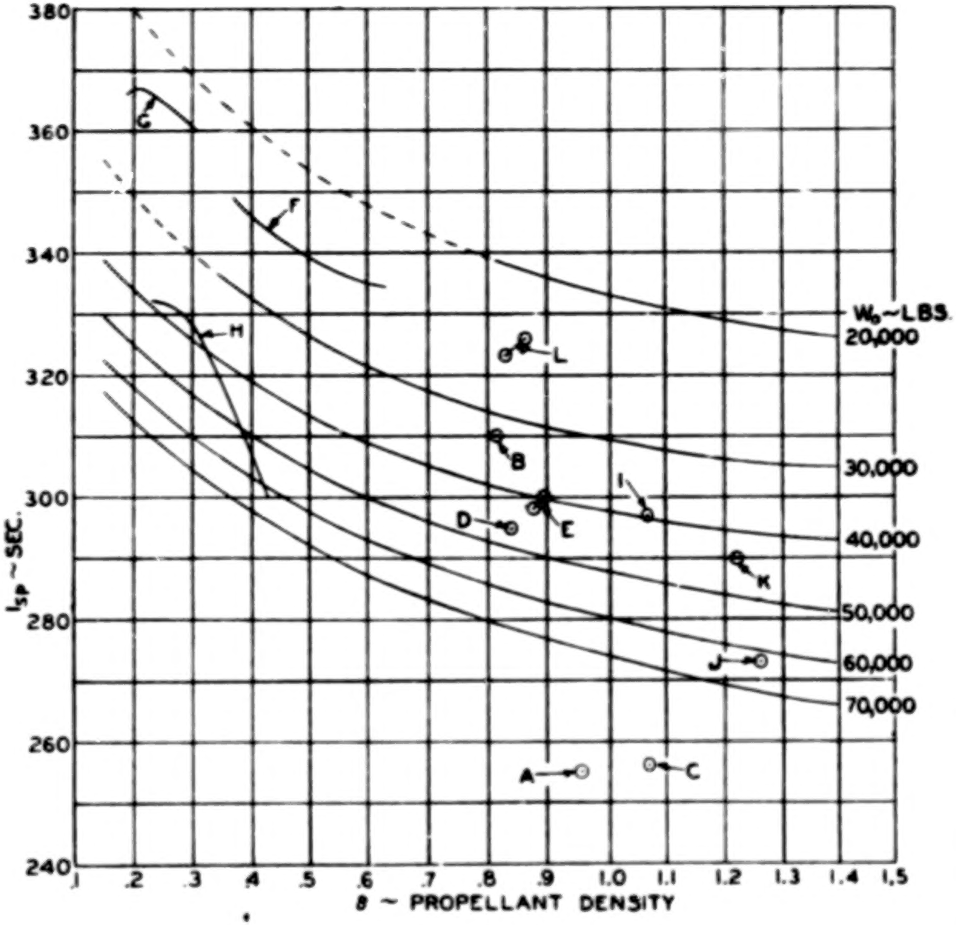
Among the report's conclusions:

1) If it were desired to build rockets for extremely long range, i.e., 15,000 miles to infinity, it would be necessary to achieve vehicles with high mass ratios. In this type vehicle, specific impulse is of greater importance than is propellant density.
2) The minimum size vehicle capable of satellite performance varied from about 25,000 to 60,000 pounds gross weight for known liquid propellants.
3) The best propellants for satellite vehicles based on minimum gross weights would be:

	Propellant	Minimum Gross Weight, Pounds
1.	O_3 and H_2	23,000
2.	F_2 and H_2	25,000
3.	F_2 and Li	25,000
4.	O_2 and $Al(BH_4)_3$	33,000
5.	O_2 and H_2	38,000
6.	F_2 and N_2H_4	40,000

VII. THE PUMPING OF LIQUID HYDROGEN

In 1946 Dietrich Singelmann, a German rocket-engine designer employed by the Air Material Command, revealed the unique characteristics of a centrifugal pump



ASSUMPTIONS:

1. $K_o = 2\ g$
2. DRAG DATA FROM A - 4
3. MOTOR DESIGNED FOR 50,000 FT.
   ($P_c = 500$ $\gamma = 1.2$)
4. ORBITAL ALTITUDE = 300 MILES
5. PAY LOAD = 1000 LBS.
6. THE EFFECT OF
   a. g
   b. ALTITUDE
   c. DRAG

   ON PERFORMANCE HAS BEEN INCLUDED.
7. W_o GROSS TAKE OFF WEIGHT
8. DASHED PORTION OF W_o CONTOURS REPRESENT PROPELLANT COMPARTMENT SKIN THICK. LESS THEN .010 INCH
9. I_{sp} VALUES AS PLOTTED ARE 95% OF THEOR. * I_{sp} AT 300 P.S.I.A. P_c
10. STRUCTURE OF VEHICLE IS THIN STAINLESS STEEL SHEET STABILIZED BY INTERNAL GAS PRESSURE

* SHIFTING EQUILIBRIUM BASIS

KEY	OXID.	FUEL
A	O_2	NH_3
B	O_2	$AL(BH_4)_3$
C	O_2	N_2H_4
D	O_2	B_2H_6
E	O_2	B_5H_9
F	F_2	H_2

KEY	OXID.	FUEL
G	O_3	H_2
H	O_2	H_2
I	F_2	N_2H_4
J	O_3	N_2H_4
K	F_2O	N_2H_4
L	F_2	Li

Fig. 13
The Relative Influence of Propellant Density and Specific Impulse on the Size of a Satellite Vehicle

consisting of a light-weight radial vane rotor with a concentric case that he had developed for the oxidizer pump of the BMW 109-718 Booster Rocket Engine manufactured by the Bayerische Moteren Werke. The rotor of this pump (Figures 14A and 14B), consisted of a hub with three or more radial thin metal vanes. A partial shroud extending to perhaps 1/3 of the radius, was used on the back side of the vanes. No forward side shroud was used. The discharge of the pump was taken through two or more round tangential orifices whose diameter determined the discharge characteristics of the pump. Dr. Singelmann described one pump in particular which operated at 25,000 rpm and developed 82 atmospheres pressure with nitric acid. The impeller was 75 millimeters diameter by 12 millimeters wide at the tip and pumped 7 liters or 25 lbs/sec at an efficiency of 45 percent. This same pump developed about 75 atmospheres pressure with water.[31]

Subsequently, the group at Ohio State University Research Foundation No. 264, constructed such a pump based on Singelmann's designs under Contract W33-038 ac-14794 (16243). The results were discussed during a visit by Aerojet personnel in November 1947.[32] Due to the extremely low temperature of liquid hydrogen, the Ohio State Group had found it necessary to provide some method of insulating the pump from room temperature. It appeared practical from the test standpoint to use a large vacuum jacket tank and submerge the pump in the particular fluid being handled. Results of pump tests with water and liquid nitrogen were good, but with liquid hydrogen the pump cavitated most of the time. This was caused by an excessive heat leak into the pump resulting from incomplete submersion. The rapid evaporation of the liquid hydrogen in the submersion tank made maintaining an adequate liquid level extremely difficult. Nevertheless some of the points seemed to fall on the anticipated head flow curve at higher flows.[33]

A subsequent interview with Singelmann after the Ohio State visit resulted in recommendations for pumps useful to the XLR16-AJ-2 engine program.[34] During the following month, it was learned from an interview with M. N. Nyborg of the Naval Air Missile Test Center, Point Mugu, that two pumps from the BMW 109-718 Jato Unit (for the ME 262 airplane) could be obtained to provide an early test on the basic characteristics of such a design.[35] Performance tests with water of this 3.3-inch-diameter oxidizer pump revealed that the characteristics of this pump were superior to those of conventional centrifugal pumps in the same speed range. Of greatest interest was the constant flow characteristics when operated at reduced head.[36] Subsequently performance and cavitation tests with water were conducted with the Aerojet designed, AR-2720, a 6-inch diameter, shroudless, radial vane, centrifugal pump with a conical discharge diffuser.[37]

The impeller, bearings, and seals of the AR-2720 pump were modified for testing with liquid hydrogen, and the pump reidentified as AR-2797. This pump incorporated an impeller machined from a single billet of 17ST aluminum alloy. The bearings used in the

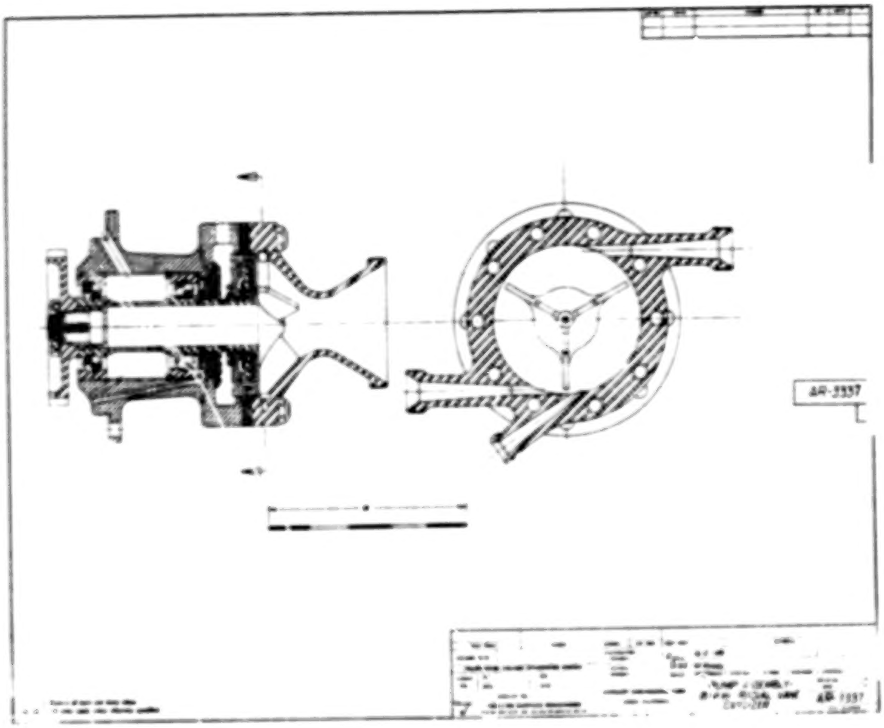


Fig. 14A
Drawing of BMW Radial Vane Oxidizer Pump Assembly

Fig. 14B
BMW Oxidizer Pump Disassembled

pump were deep-groove ball bearings having bronze cages. Micarta clearance seals were used in the pump for hydrogen testing. Provision was made for pressurizing the seal section with helium in order to minimize hydrogen leakage from the impeller housing. The construction of the pump is shown in Figure 15.[38] The pump was designed to the following specifications:

N = 40,000 revolutions per minute
H_{max} = 23,300 feet
Q_{max} = 87 gallons per minute

Difficulty was experienced in cooling the pump to a temperature low enough to permit filling the pump case with liquid hydrogen. This was caused by the small size of the diffuser through which the gas generated during cooling had to escape. This trouble was eliminated by adding four 1/4-inch-diameter holes in the pump case, and connecting these to a bleed valve which was closed after cooling the pump and before starting the test run. With this difficulty overcome, liquid hydrogen was pumped successfully. A flow rate of 0.68-lb per second and a pressure rise across the pump of 400 psi were obtained. The head-vs-capacity curve is shown in Figure 16. This curve shows the unique property of this type of pump: that of operating at a constant capacity over a wide range of pressures, which in this case was from 40 percent or lower up to 85 percent of the maximum pump-pressure rise. This unique property offered advantages when used in a rocket engine pumping plant. The flow rates could be controlled by the pumps, eliminating the need for flow-regulating valves and resulting in a basically simpler and lighter engine.

For the AR-2797-type pump, the head varied approximately as the square of the speed while the capacity varied directly as the speed. At the design speed of 40,000 rpm, and the corresponding peripheral velocity of 1,047 feet per second, the head would be in excess of 20,000 feet and the capacity would be approximately 86 gallons per minute. These results were in approximate agreement with the predicted values. The power limitation of the pump test stand prevented testing at 40,000 rpm, the design speed for the pump. The pump shown in Figure 17 was operated successfully with as little as 25 psi difference between this suction pressure and the vapor pressure. This indicated that a suction pressure as low as 40 psia could be used in a high-altitude test vehicle or missile in which the liquid hydrogen reached the pump inlet with a vapor pressure of approximately 15 psia.

The pumping of liquid hydrogen was demonstrated successfully for the first time with the lightweight, high-speed centrifugal pumps, in a series of tests conducted during March 1949.[39]

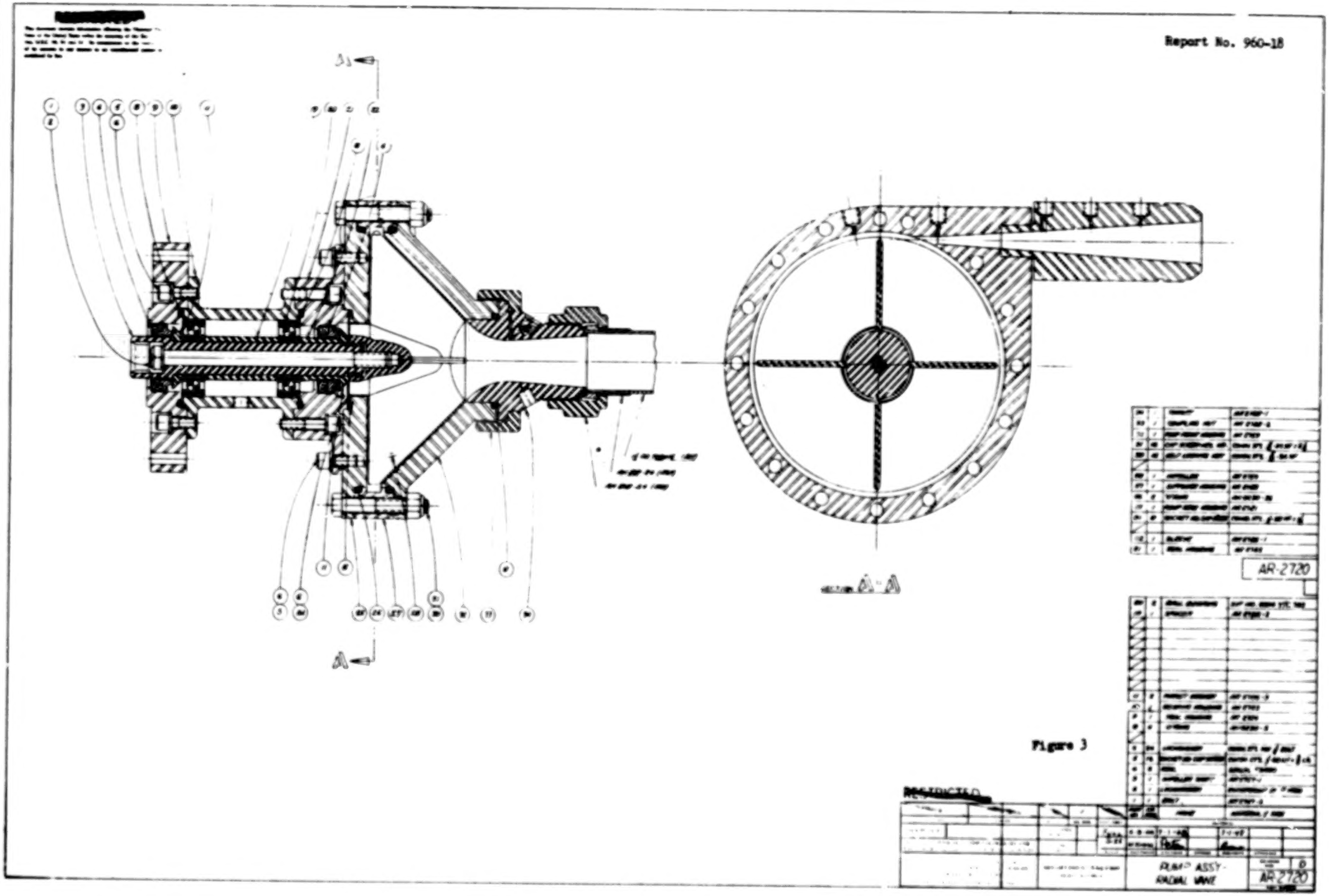

Report No. 960-18
A
A
SECTION A-A
AR-2720
Figure 3
RESTRICTED
PUMP ASSY - RADIAL VANE
AR-2720
D

Fig. 15
Drawing of Radial Vane Pump Assembly

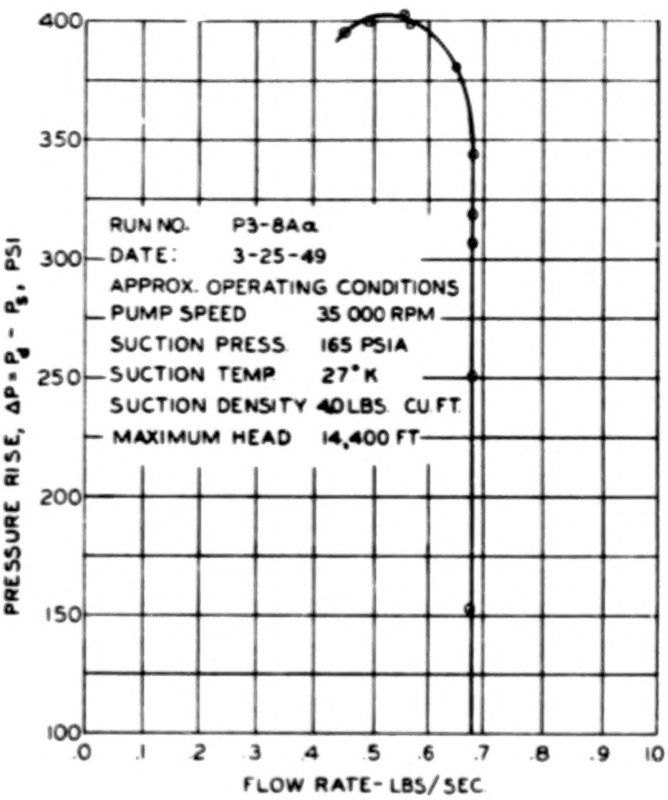


Fig. 16
Performance Curve of Liquid-Hydrogen Pump (Aerojet)

VIII. THE EFFECT OF JET OVEREXPANSION AND SEPARATION ON ROCKET THRUST CHAMBER PERFORMANCE

Early theoretical analysis had indicated that a rocket engine designed correctly for high altitudes (low ambient pressures) would suffer serious losses in performance when operated at low altitudes (high ambient pressures).[40] The results (Figure 18) indicated that although the performance of a sea-level motor was 20 percent less than that of a high-altitude motor, the high-altitude motor would lose 95 percent of its performance if operated at sea level. This assumed complete overexpansion in the high-altitude rocket engine. The PTV vehicle would have to use a high-altitude version of the XLR16-AJ-2 engine in order to obtain high performance. However, this engine usually would be started at sea level and would have to furnish sufficient thrust to lift the vehicle. If the

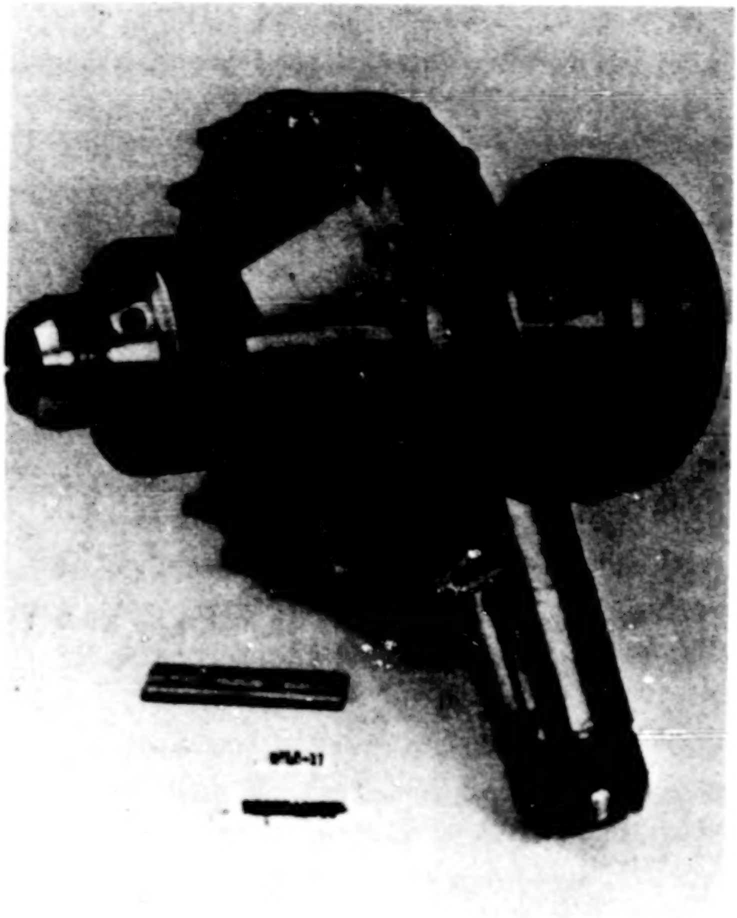


Fig. 17
Aerojet Liquid-Hydrogen Pump

losses were as great as calculated, it would be necessary to develop some means of controlling the excessive overexpansion which causes the losses in performance shown in Figure 18.[41]

A flared tubular thrust chamber with a 33:1 area ratio nozzle was constructed for tests to determine the loss in thrust chamber performance caused by overexpansion in the chamber pressure region from 150 to 350 psia. The thrust chamber appears in Figure 19. Twenty tests were made with the chamber. In addition, sixteen runs were made on an identical chamber except that a sea level (16:1) area ratio nozzle was employed. Performance of the overexpanding-nozzle test thrust chamber and the normal-area-ratio thrust chamber at mixture ratios of 4:1 and 3:1 under the same operating conditions were essentially the same, thus, the chamber parameter c* was not affected. Therefore, the nozzle parameter C_F was affected and the overexpansion losses occured only in the nozzle.

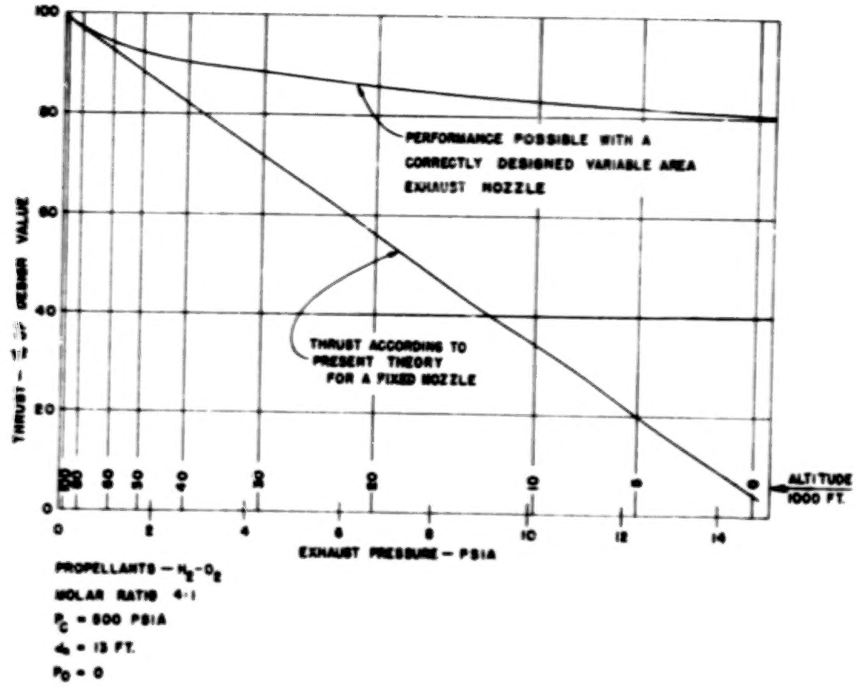


Fig. 18
Performance at Low Altitude of a Nozzle Designed for High Altitude

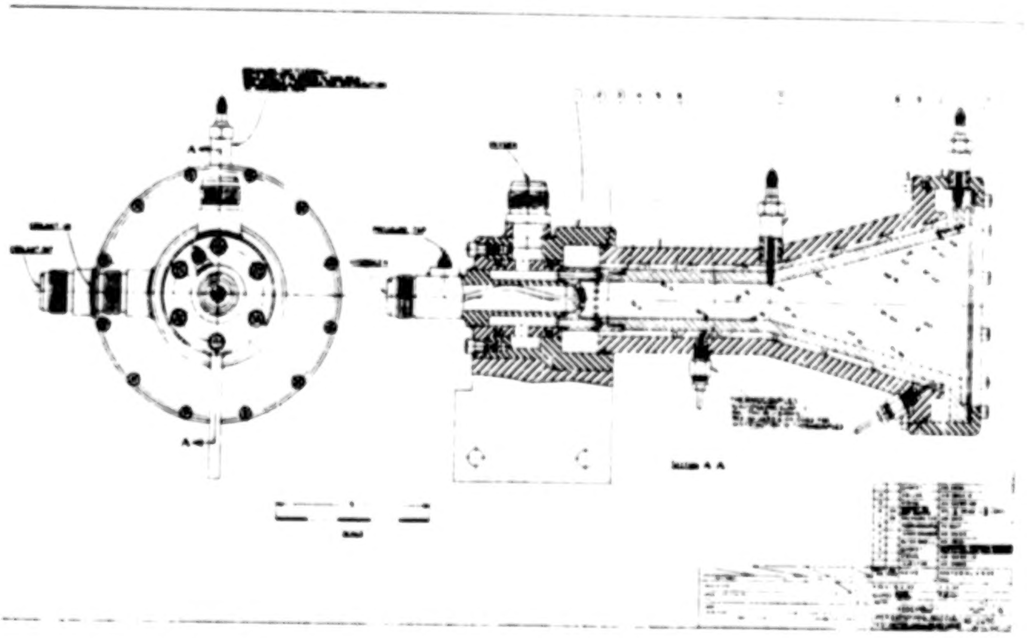


Fig. 19
Drawing of Overexpanding-Nozzle Thrust Chamber

It appeared from these tests that the losses in the overexpanding-nozzle thrust chamber were about 13 percent greater than in a normal expansion-nozzle thrust chamber.

It was apparent that the problem of minimizing losses would still remain to be solved for single-stage, long-range rocket missiles. The conceptual solutions were easily formulated, and involved the injection of gases into the side wall of the nozzle to cause separation at the correct area ratio. However, at this point in the contract, it was considered undesirable to continue to divert manpower for this investigation.

IX. LIQUID HYDROGEN-LIQUID OXYGEN THRUST CHAMBER DEVELOPMENT

The object of this portion of Contract NOa(s)-8496 was to develop a 3000-lb-thrust regeneratively cooled rocket thrust chamber which would satisfy the target specifications for the rocket engine described in Section V. An over-all engine I_{sp} (specific impulse) of 303 seconds was required, with estimates that the turbopump would require six percent of the total propellant flow. Consequently, the thrust chamber would have to produce an I_{sp} of 322 seconds. A further requirement, based upon the tankage of the missile, called for the H_2:0_2 molar ratio for the thrust chamber to be only 4:1.[42]

The design of a regeneratively cooled 3000-lb liquid hydrogen-liquid oxygen rocket engine demanded that certain new design data be established because all earlier work had been conducted with gaseous propellants. This data was to be obtained from a 400-lb thrust chamber development program planned in two phases. Phase One would determine the effect of motor geometry and injection configuration upon heat-transfer and performance. These tests were to be made without the benefit of film cooling by liquid hydrogen. Having determined a suitable thrust chamber geometry and best injector configuration, Phase Two was to be initiated to determine the effects of film cooling by liquid hydrogen upon performance and heat transfer. The film coolant was to be injected down the chamber walls from the injector face and/or in the chamber just forward of the nozzle. Adequate performance, as noted above, was an I_{sp} of at least 322 seconds. To satisfy performance and weight requirements and to permit regenerative cooling with liquid oxygen, it was decided that motor testing should be at 500 psia chamber pressure.

The best injector and chamber configuration developed on the 400-lb-thrust program would be used as a basis for the design of the 3000-lb combustion chamber and injector. Development of the 3000-lb-thrust chamber was to continue until target specifications had been achieved. But time and funds did not permit the completion of the systematic program outlined above. Instead, an abbreviated program, described in the following paragraphs, transpired.

The first test of the 400-pound-thrust unit was conducted on January 21, 1949. By May 5, 1949, sixteen tests had been completed at 400-lb thrust, using liquid-phase propellants and water-convection-cooled, liquid hydrogen/liquid oxygen combustion chambers with suitable nozzles to produce 400 pounds nominal thrust at 300, 400 and 500 psia chamber pressures. The general construction of these combustion chambers and nozzles are shown in Figure 20. The inner chambers and nozzles were made of copper to permit large heat-flux densities to be handled with safety. The above chambers were of different lengths and two different diameters; hence L*, length, and diameter could all be used as parameters. A typical thrust chamber mounted on the test stand is shown in Figure 21. Five injector configurations were tested with a water-cooled plenum chamber of 64 L*. The best performance was obtained with the multi-tube concentric-orifice injector (coaxial) designed by G. H. Osborn (Figure 22).

In this design, the liquid oxygen was injected through the annular orifices and the liquid hydrogen through the central tubes. Each pair of orifices supplied propellant corresponding to about 5 pounds of thrust. Film cooling was again supplied by 24 0.015-inch-diameter orifices, which sprayed liquid hydrogen down the wall of the combustion chamber. The first run with this injector was very successful. The test lasted for 40 seconds without damage to the thrust chamber. This injector, at 504 lb/in^2 chamber pressure, gave a specific impulse of 366 seconds and an average heat-flux density of 6.45 Btu/in^2 in the chamber, and 11.3 Btu/in^2 in the nozzle. The appearance of the jet during this run is shown in Figure 23.

The 3000-lb combustion chamber (Figure 24) was designed and assembled using some parts from another program. The inner chamber was machined from a cast-copper billet. Electrolytic copper was preferred, but could not be obtained in the required size. The cast copper that was used for the inner chamber has only approximately one-third the strength and one-half the thermal conductivity of electrolytic copper. The inner and outer chamber before assembly is shown in Figure 25. This chamber assembly of 49 L* was designed with a nozzle exit to throat area ratio of only 4:1 because of size limitations of the existing parts. The helical coolant passage was designed to accomodate a water flow of ten pounds per second with a 110 psi pressure drop.

Successful tests with the multi-tube concentric-orifice injector at 400-lb thrust resulted in the 3000-lb-thrust concentric-orifice injector design (Figure 26). Four hundred and eighty-nine concentric orifices were used in addition to 60 hydrogen-film-coolant orifices. Each pair of orifices supplied propellant corresponding to about six pounds of thrust. The trouble experienced with the burning on the face of some of these injectors led to the conclusion that the fuel should be injected through annular orifices. Accordingly, the 3,000-lb injector was designed with the liquid hydrogen flowing in the annulus and the liquid oxygen flowing through the central tubes. Figure 27 shows this injector after fabrication. Figure 28 shows a water-flow test.

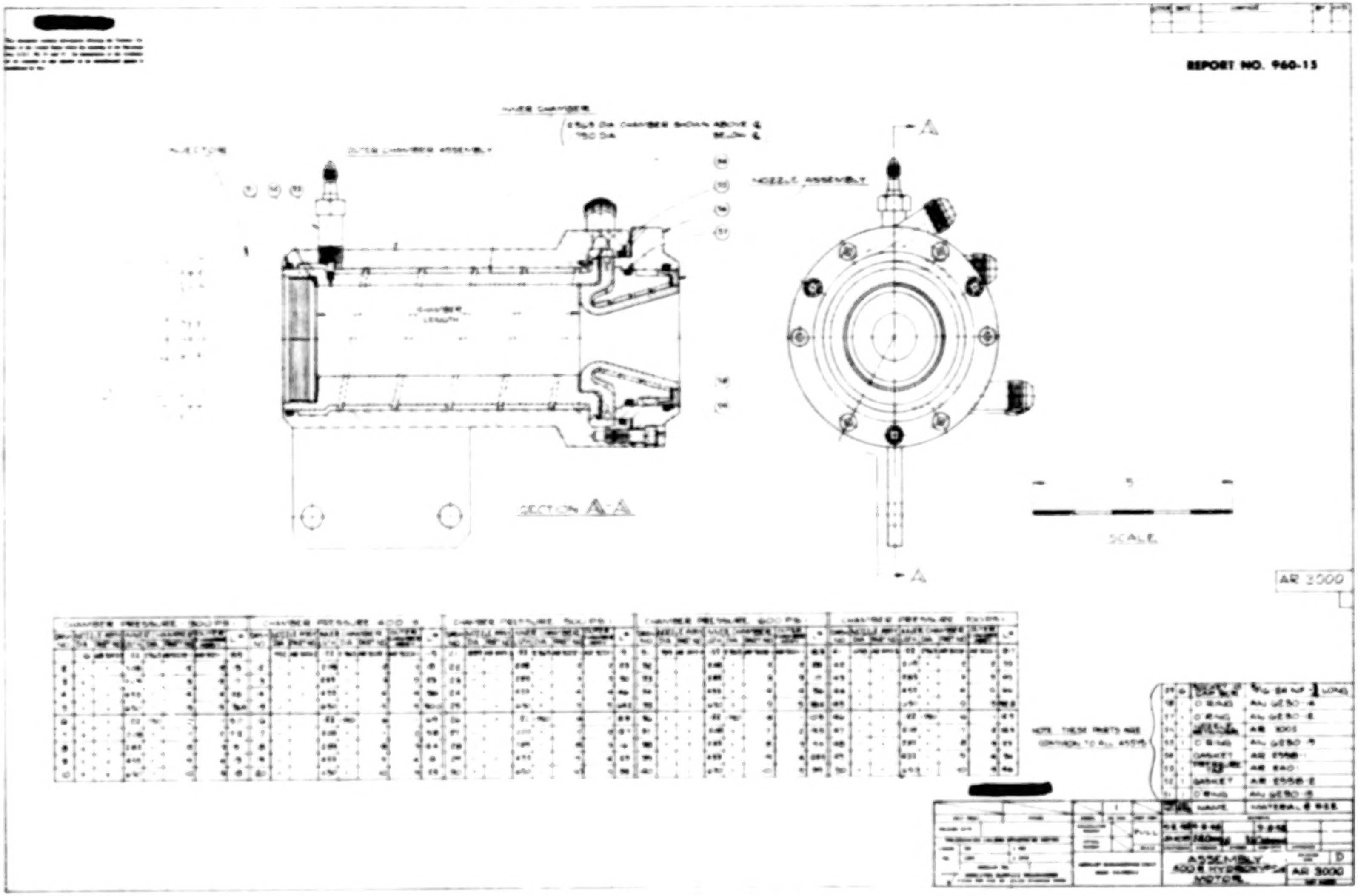

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Fig. 20
Drawing of 400-Pound-Thrust Liquid Oxygen/Liquid Hydrogen Thrust Chamber

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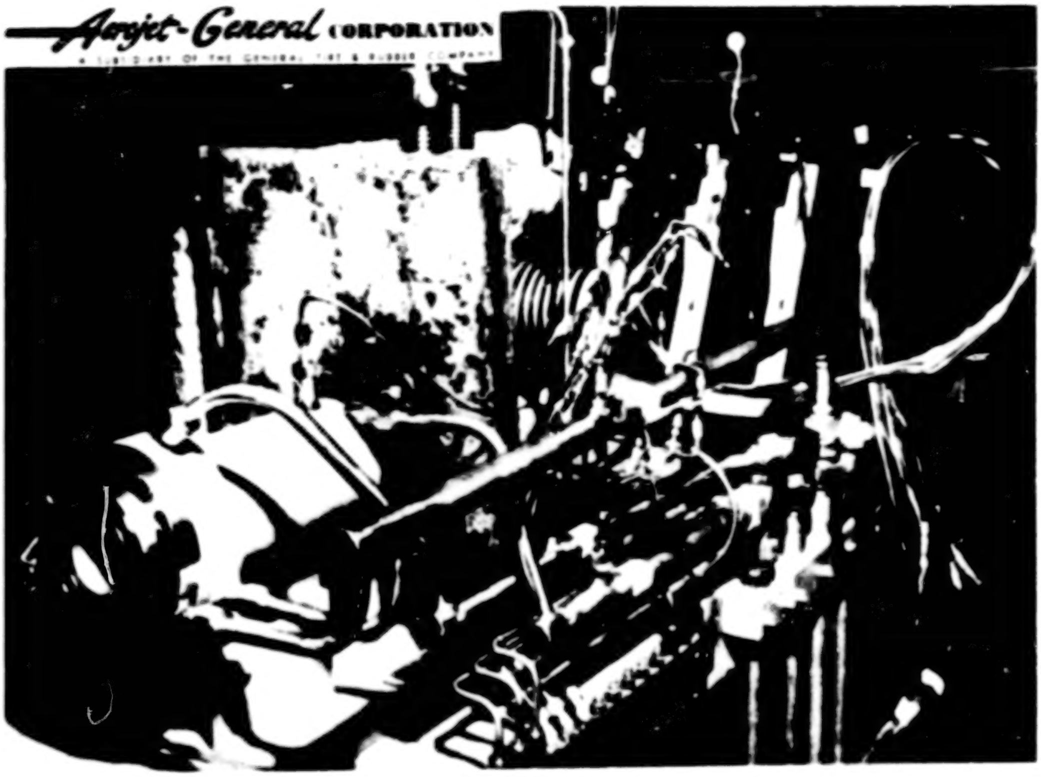

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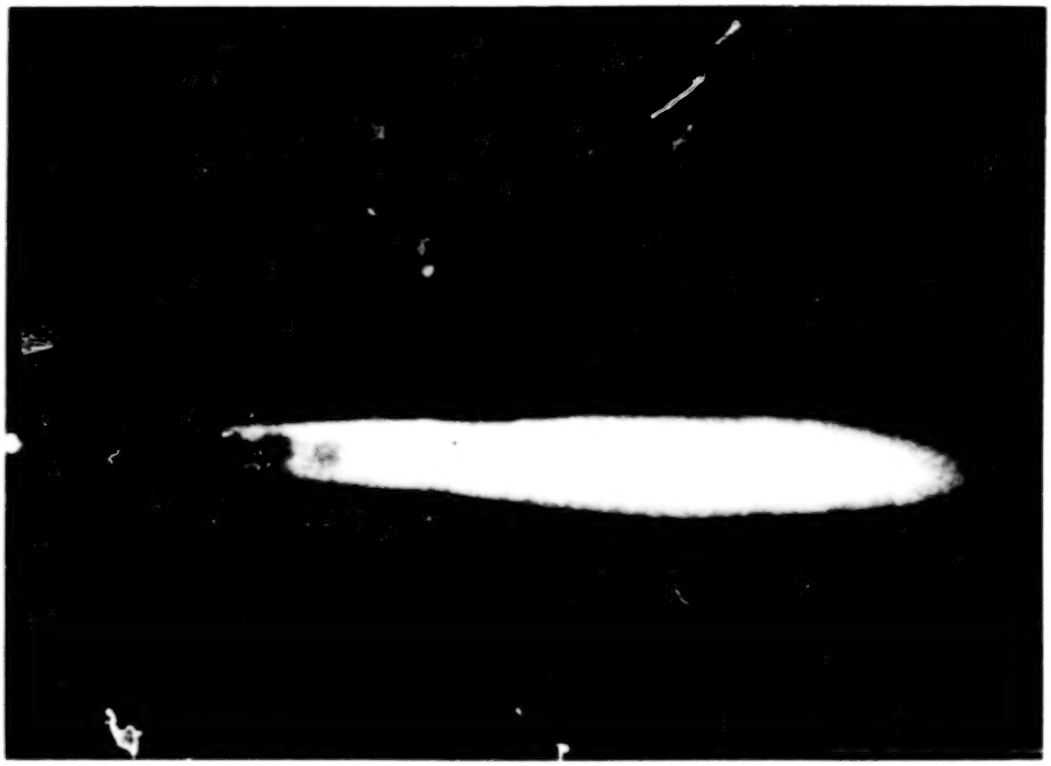

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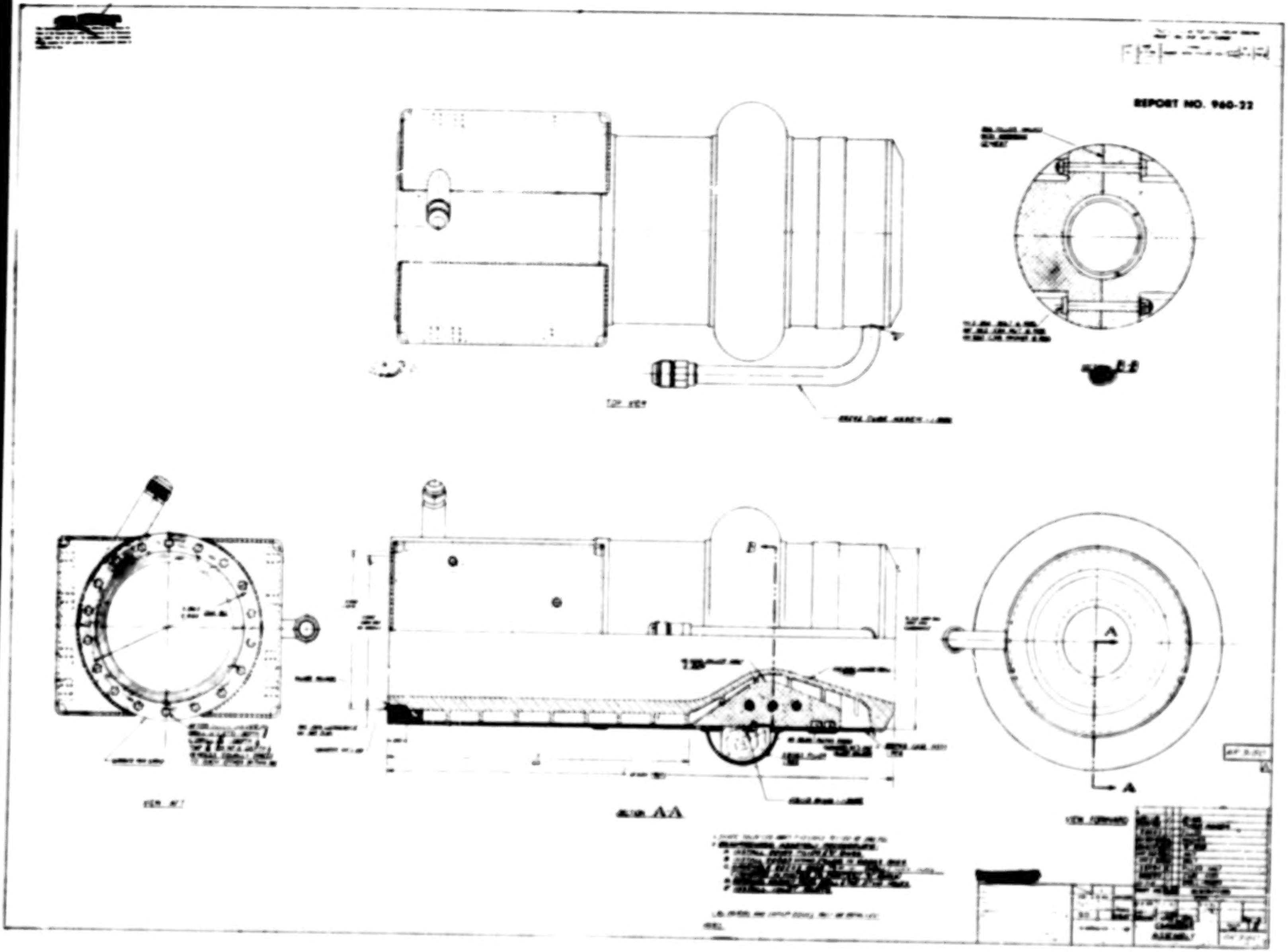
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Fig. 24
Drawing of 3000-Pound-Thrust Liquid-Oxygen/Liquid-Hydrogen Thrust Chamber

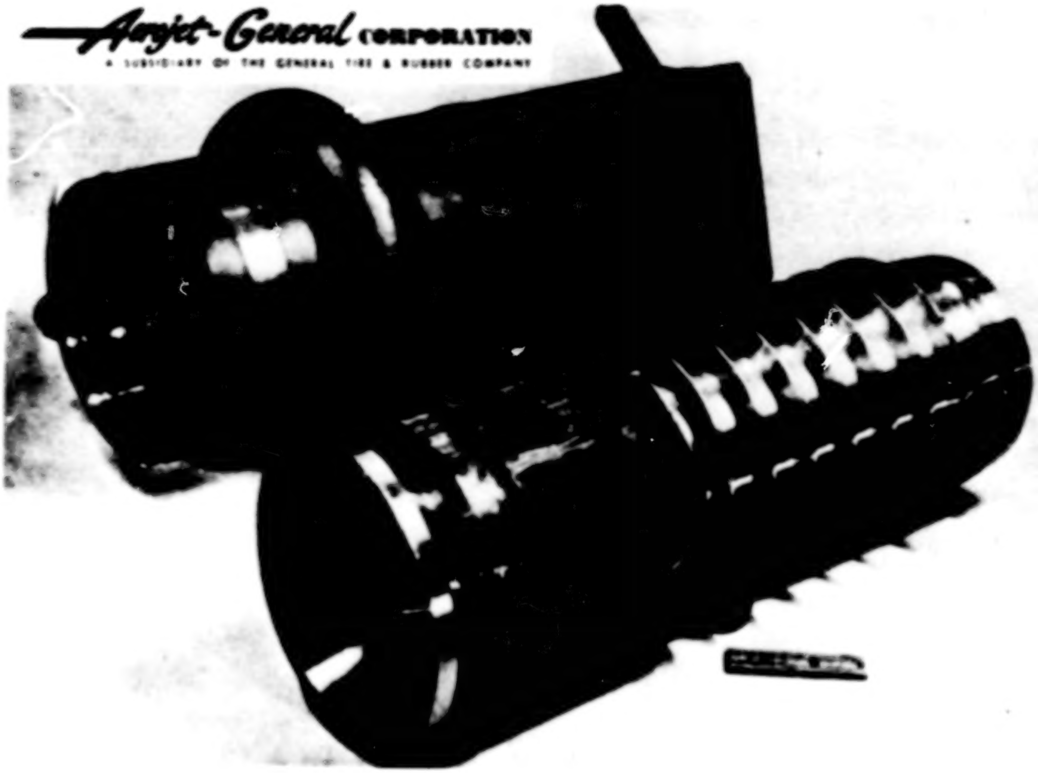
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Fig. 25
3000-Pound-Thrust Chamber With Cast-Copper Inner Chamber

CONCLUSIONS

By August 1949, the liquid hydrogen-liquid oxygen program at Aerojet had demonstrated the feasibility of virtually all the components in present-day, high-energy, liquid-rocket engines. Transpiration and film-cooled thrust chambers had successfully operated. The first liquid-hydrogen tests of the coaxial injector had been conducted. The first pump to successfully produce high pressures in pumping liquid hydrogen had been tested. A 1000-lb-thrust gaseous propellant and a 3000-lb-thrust liquid-propellant thrust chamber had operated satisfactorily. And the first tests had been conducted to evaluate the effects of jet overexpansion and separation on performance of rocket thrust chambers with hydrogen-oxygen propellants. The production and handling of liquid hydrogen had been demonstrated to be practical and less hazardous than originally believed by industry spokesmen when the program started. These pioneering investigations established a valuable engineering background. However, the application of this technology had to wait for almost a decade before finding use in high-energy upper stages for the space program in the 1960s.

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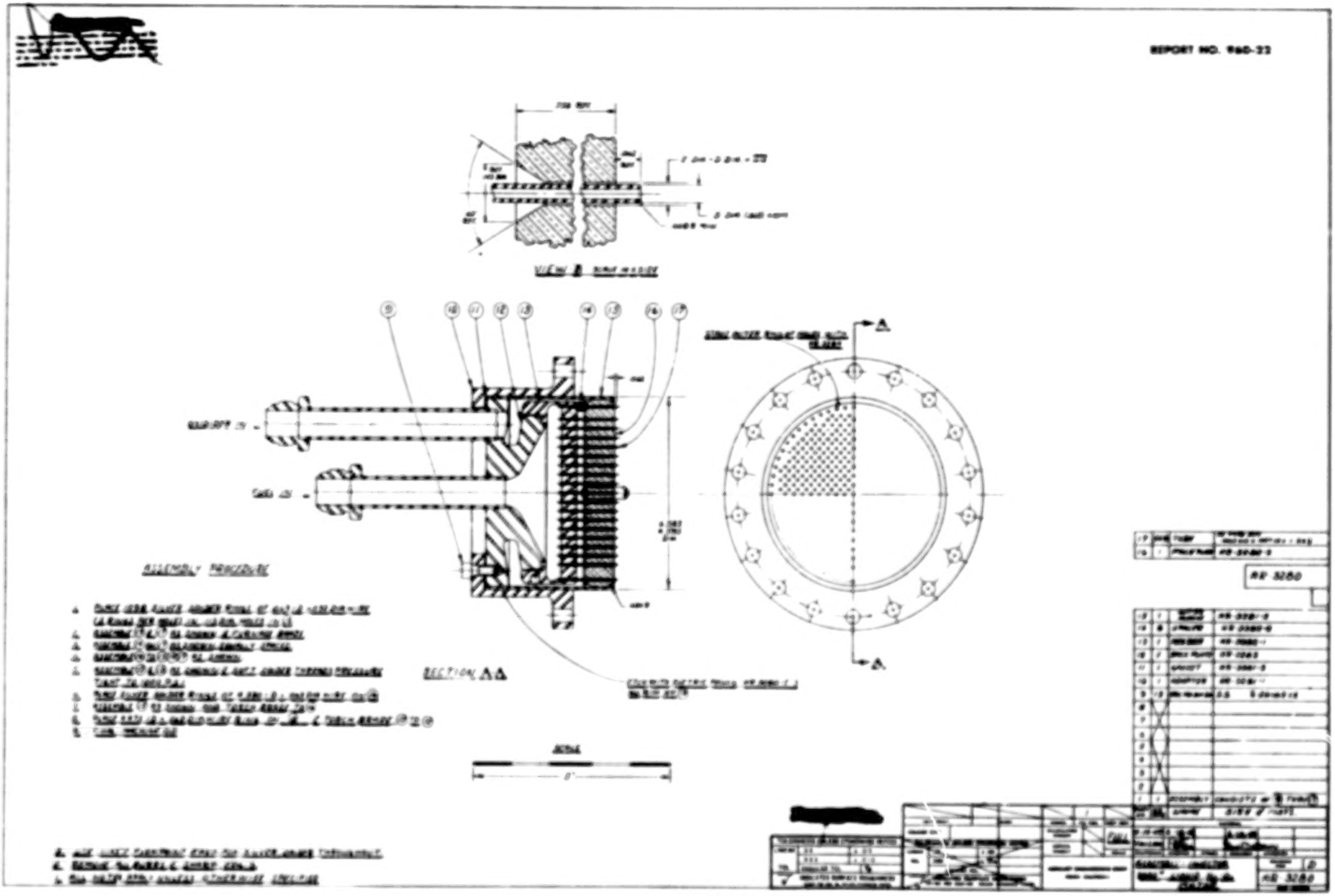
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Fig. 26
Drawing of 3000-Pound-Thrust Liquid-Oxygen/Liquid-Hydrogen Coaxial Injector

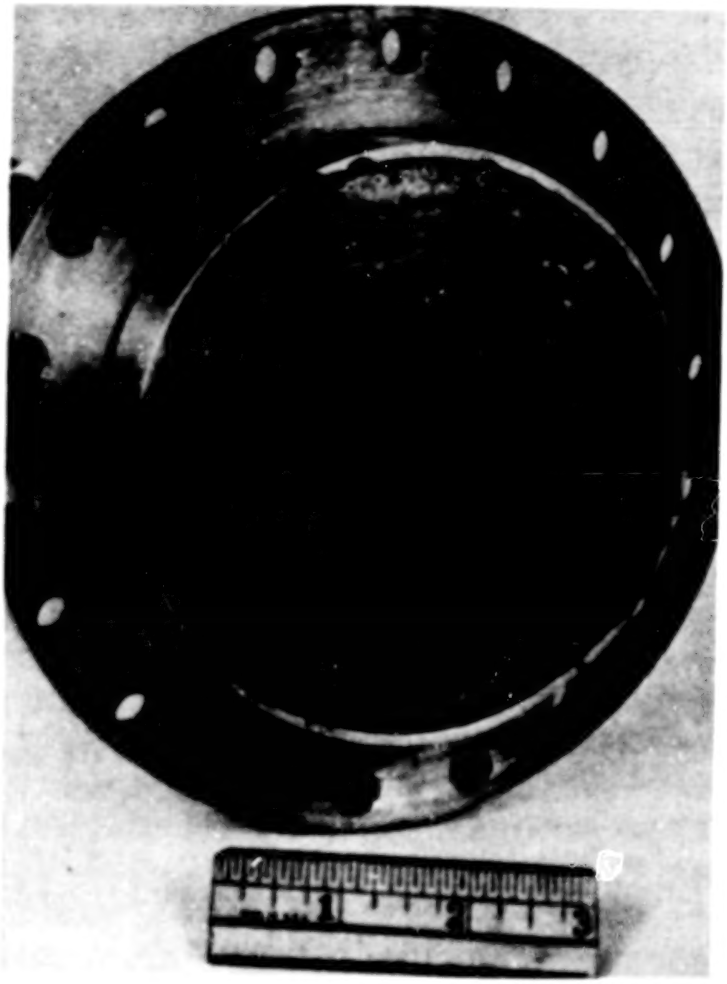
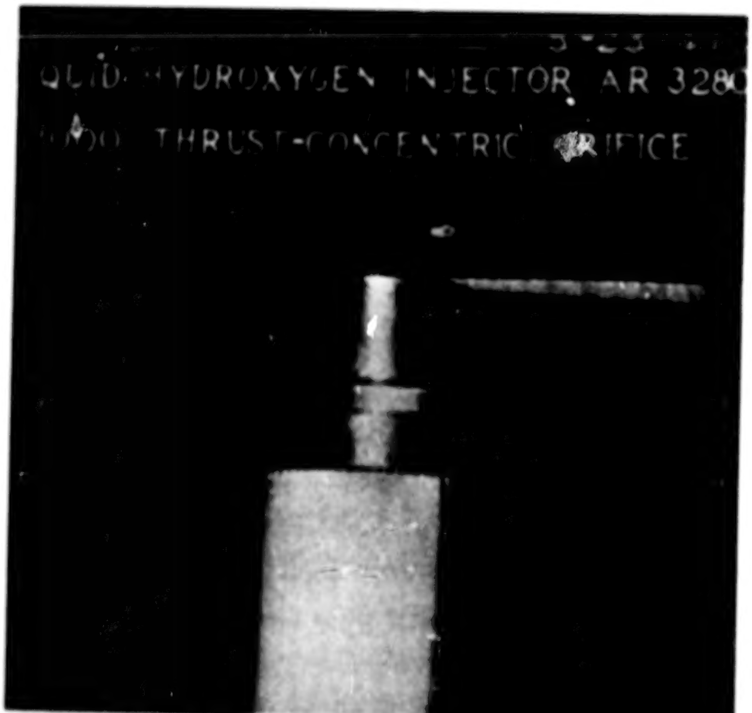


Fig. 27
Assembled 3000-Pound Thrust
Coaxial Injector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QUID HYDROXYGEN INJECTOR AR 3280
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Fig. 28
Water Flow Test of 3000-Pound-
Thrust Coaxial Injector

Fig. 29
3000-Pound-Thrust Chamber
Assembly During Second Test

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A STONE'S THROW INTO THE UNIVERSE: A MEMOIR[+]

F. Zwicky (Switzerland)[++]

Ten days after I first tried to launch a man-made object into interplanetary space with a V-2 rocket from White Sands, New Mexico, E. B. White in The New Yorker of December 27, 1946,reported:

> The year ends on a note of pure experimentation. Dr. Fritz Zwicky last week tried to hurl some metal slugs out into space, free of the Earth's gravitational pull. Dr. Zwicky stood in New Mexico and tossed from there. He was well equipped; he had a rocket that took the slugs for the first forty-mile leg of the journey and then discharged them at high velocity to continue on their own. The desire to toss something in a new way, or to toss it at a greater distance, is fairly steady among men and boys. . .[1]

Author White had guessed correctly. As a boy in the Swiss Alps I had thrown stones across rivers and snowballs up church steeples as far and as high as I could, without having anything particular in my mind, except to be the best. My attempt to throw stones (artificial meteors) away from the Earth, however, was to be just a first step in a more purposeful chain of events. Col. Rivkin asked me at White Sands in December 1946 just what my purpose was, I responded, "First we throw a small slug into space, then a bigger one, then a shipload of instruments, and finally ourselves."[2]

The projects I had in mind for extraterrestrial space research date essentially to 1928, when George Ellery Hale, for whom the entire complex of the observatories of the California Institute of Technology and the Carnegie Institute of Washington is named, obtained a grant of six million dollars for the construction of the 200-inch telescope and the necessary auxiliaries. At that time, Sinclair Smith, John Strong, and myself, all physicists at the California Institute of Technology, shifted our principal scientific activities from physics to astrophysics and observational astronomy, fields that eventually included the use of both optical and radio telescopes. Following Hale's advice to "make no mean plans," we later extended our studies to include many instruments that could be carried aloft by balloons,

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[+] Presented at the Fifth History of Astronautics Symposium, International Academy of Astronautics, Brussels, Belgium, September 1971.

[++] California Institute of Technology and Hale Observatories.

Earth-circling satellites and space ships, including preparations for an observatory on the Moon, possibly in 1975, a proposal supported by the Lunar International Laboratory Committee (LIL) of the International Academy of Astronautics.

Vehicles that could carry instruments to great heights became available as a result of the technical developments of World War II. I participated actively in the war effort as director of research for the Aerojet Engineering Corporation, as a member of the Scientific Advisory Board of the Army Air Forces, and as one of its technical representatives in 1945 and 1946 in Germany and Japan. Thereafter, by a most improbable but fortuitous chain of circumstances and events extending over three decades, I realized a number of divergent projects and goals that I had thought about for a long time. These projects involved problems in basic astronomy, physics, chemistry, engineering, international law, and ways to bridge the ever-widening gap between science, technology, and the general public. In this brief memoir I will only touch upon a few of the high points of my experiences in the fields of astronomy, the development of propulsive power, and man's march into space. A more detailed account of these events will appear in one of my forthcoming books, Operation Lone Wolf.

Specifically, the projects I set for myself involved:

1. A morphological survey of all the results, scientific, technical, and human, that might be achieved in man's MARCH INTO INNER AND OUTER SPACE. Outer Space here stands for extraterrestrial space. Inner Space refers to the depths of the oceans, to the interior of the Earth, as well as to the interior of all the other members of the Solar System, or, for that matter, of any cosmic bodies that might eventually come within man's reach.[+]

[+] I shall not go into any details about the many problems concerning inner spaces, except to mention that we surveyed and discussed all of the potential propulsive power plants and propellants, the possible means of traveling through, communicating and living in, as well as exploring and exploiting inner space. At the Aerojet Engineering corporation in Azusa, California, we studied hydroturbojets[3] in particular, as well as the synthesis of effective hydrofuels, and laid the groundwork for the future mastery of the depths of the oceans. In this connection I should mention that my former teacher in physics, 1916-1920, Professor Auguste Piccard, at the Federal Institute of Technology in Zurich, after he had made the first flight into the stratosphere in 1931, also pioneered the first dives into great depths of the oceans. These efforts reached their climax in the dive of the bathyscaph "Trieste", piloted by his son Jacques Piccard to the greatest oceanic depth of over 11 km in the Challenger Bottom near Guam in the Pacific.

In the early 1940s I proposed designs for terrajet engines, that is, propulsive power plants intended to move through the solid Earth, that still await construction and application. These engines will be vital not only for terrestrial uses, but in particular for operating underground on the Moon and on other bodies in the Solar System.

2. Develop effective means to reach and occupy all humanly attainable locations in Outer Space and in Inner Space.
3. Revised approaches to Astronomy. In the past, astronomy was entirely a passive science that had to rely solely on observations of the incoming messengers such as light and corpuscular radiations from the extraterrestrial spaces and bodies. But in the 1940s, witn the visualization and realistic planning of vehicles capable of traveling through extraterrestrial space, three new major types of activities promised to become possible in the near future, namely:
    a. The direct exploration of extraterrestrial bodies and space.
    b. Experimental Astronomy, in the sense that extraterrestrial bodies, or at least selected parts of them, would be directly investigated to test hypotheses.
    c. Reconstruction of the universe, or at least a very small part of it, that is, for the time being the Solar System.[4]

These goals I had clearly in mind as non-military post-war projects at the time I was working at the Aerojet Engineering Corporation on the invention and construction of various types of propulsive power plants and the many auxiliary devices and weapons that would help bring down the facist regimes led by Hitler, Mussolini, and the Japanese war lords.

EXPERIMENTAL ASTRONOMY

I have long been concerned with subjecting cosmic bodies to direct tests, similar to experiments on terrestrial matter in the laboratory. Concerning my first modest efforts in this direction, J. C. Pecker, Director of the Nice Observatory[5] observed: "The first realistic proposal of experiments of this type was no doubt that of F. Zwicky. Zwicky proposed to produce artificial meteor showers. His first attempts in 1946 were unsuccessful—but later experiments succeeded in 1957--and since then other authors have continued analagous work."

The goals in this field that I had in mind at the end of World War II were:

1. Launching fast particles (artificial meteors) at all heights in the atmosphere for the purpose of exploring its physico-chemical conditions, for instance, winds, shock waves, eddies, desities, electric and magnetic fields as well as local chemical composition. Data on all of these characteristics could be obtained through observations of the tracks and point-for-point spectra of the artificial meteors.[3]
2. Launching artificial meteors from great heights with velocities greater than 11.2 km/sec into interplanetary space, free of the gravitational pull of the Earth.

3. Bombarding the Moon on the dark side of the terminator with ultrafast particles containing reducing metals such as Al, in order to determine the presence of water of crystallization in deeper lying rocks on the Moon's surface. Bound H_2O in the crystals, analogous to that found in granit, could be extracted by the life-sustaining solar furnace that I designed and tested at Aerojet in the 1940s.[3] If no such water of crystallization is found at any reasonable depth, the protons of the solar wind entrapped in the surface grains will have to be used for the production of water, propellants for hybrid propulsive power plants, and foodstuffs on the Moon.[6, 7]

Curiously enough, in the 1940s little or no enthusiasm could be engendered anywhere for these simple projects, and practically no funds could be mustered for their realization. During almost two decades, Mr. J. Cuneo, the former patent attorney at Aerojet, and myself, carried on with our own means, working in our free time whenever we could. Some noteworthy results nevertheless were achieved, since fortuitously, the US Army Ordnance, as well as the US Navy and Air Force, made various of their facilities available to us—for which sincere thanks are due them. Some of the developments have already been described.[3] Additional developments are briefly being touched upon below.

PHASE I

As the most convenient means to produce fast particles, in 1945 we decided to use shaped charges of cast and putty explosives, as well as JET-X (that is, liquid nitromethane doped with diethylamine which we developed at Aerojet and which, because of its uniformity, gave the finest ejected particle jets, as shown in one of my previous LIL Symposium articles)[6] As inserts we generally used copper or iron cones with an apex angle of sixty degrees. With these, particle velocities of nearly 10 km/sec were obtained[3, 4] as measured by rotating cameras or rotating hexagonal mirrors plus stationary cameras. With very great difficulty later on we produced conical inserts of titanium because of its ready oxydizability. These ejected particles of Ti, "burning" in the air, produced very luminous tracks, even in tenuous parts of the atmosphere, where poorly oxydizable particles like iron and copper could not be seen, since friction heating did not suffice to make them sufficiently luminous. The problem, however, arose over how to produce self-luminous particles that could be seen even when ejected into a vacuum. This problem I solved through the invention of coruscatives or heat explosives.[3]

Coruscatives are solids that detonate or explode on application of sufficiently strong shearing stresses for instance, but in contradistinction to common explosives, the reaction products at Standard Pressure and Temperature (SPT) are solids. Virtually no gas is generated, or only small amounts of gases are generated. This of course was an imperative condition for our purposes, otherwise we would have ejected a dispersed swarm of particles and gas clouds instead of obtaining well defined particle tracks. I should note that most of the chemists with whom I discussed the matter scoffed at the possibility of coruscatives.

In order to have all of the reacting components distributed in molecular dispersion, it would be ideal to use single crystals of coruscatives. Such crystals exist, but unfortunately, upon transformation they do not release enough energy to heat the reaction products to sufficient incandescence for our purposes. We therefore started experimenting with highly compressed fine powders of powerful reagents. The first obvious mixture to try was thermit, the welding mixture Fe_2O_3 + 2Al which, on reaction, gives iron and aluminum oxide at a temperature of about 4000°C. But we never succeeded in making compressed slugs of thermit explode on application of high shear stresses. Several other compressed powders, however, acted as desired. Finally, we used mostly fine powders of titanium and carbon which, as a compressed conical insert in a shaped charge, can be collapsed fast enough to react itself explosively, resulting in the ejection of TiC droplets at almost 6000°C and speeds of up to 15 km/sec. A telling photograph has already been published.[6] Generally, the speed of the ejected particles can be increased by choosing smaller apex angles for the conical inserts. At the same time, however, the mass of the extruded particles dwindles disastrously, so that they become quickly unobservable.

To photograph the fast luminous particles of artificial meteors we used various types of wide angle cameras. The most powerful one consisted of an 8-inch F/1 transportable Schmidt telescope, which Russel Porter and I, with the help of instrument makers and opticians at the California Institute of Technology, built in 1939. To observe the tracks spectroscopically, single or mosaic objective transparent gratings were mounted over the full apertures of the cameras and telescopes used.[3]

To test the various shaped charges and the efficiency of our cameras, telescopes and objective gratings, over a period of fifteen years J. Cuneo and I undertook many field trips and launched the artificial meteors from the ground in the Chino Hills in Southern California, at White Sands (NM), Edwards Air Force Base (Cal), the Navy test station at Inyokern (Cal), and in the White Mountains of California at elevations above sea level from 500 m to 3500 m. Furthermore, with the help of the Office of Naval Research in Pasadena, we undertook several small expeditions, flying over to St. Nicholas Island on the California coast to launch our artificial meteors there from high-flying balloons. A long series of valuable results were obtained from all of these experiments, which are in part described in my book Morphology of Propulsive Power[3] as well as in reports by the chief scientist in the Office of Naval Research in Pasadena.

PHASE 2

At the end of World War II the US Air Force asked me to go to Germany as their technical representative to learn about various phases of military research accomplished in Germany. My findings were subsequently published in book form.[8] At the same time I and my teammates under the leadership of Col. John A. O'Mara had arranged for much of the German equipment to be sent back to the United States. Of especial interest to me and a few other scientists were a number of V-2 rockets that were brought to the White Sands Proving Grounds to be fired there with various types of scientific devices on board. For my purpose of launching artificial meteors, it was of course important that the firing occur at night. Through the good offices of my friend Theodor von Karman, General Barnes of Army Ordnance arranged for a firing of a V-2 rocket on the night of December 17, 1946.

Nine shaped charges were to be fired in groups of three at 36, 48, and 60 kilometers altitude above sea level. The flight of the V-2 rocket to a height of 190 kilometers was a beautiful one. Unfortunately, for reasons not discussed here, none of the shaped charges fired. Our disappointment was enormous. Indeed, the failure of our experiment turned out to be a disaster, because further launchings of this sort were subsequently blocked for a full eleven years. Some so-called experts on (natural) meteors, among them Professor F. L. Whipple of Harvard, reported to the cognizant agencies of the US government that the experiment which I had proposed could not possibly succeed and should not be supported. These agencies not only followed his advice, but officials actually talked about charging me with wasting the tax-payers' money. Fortunately, good friends of ours were informed that I and my collaborators had all paid our own way and had not received any funds from anybody, so we were spared the necessity of having to face a judge.

As mentioned, during the following eleven lean years, Mr. Cuneo and I continued to develop even more powerful shaped charges and better inserts to be ejected from them. We also tried to get funds for launching these larger artificial meteors into interplanetary space from sources other than any of the US governmental or military agencies. Because of their historical interest I will mention two of these attempts.

First I had the idea that, for the sake of keeping records about the beginning of experimental astronomy and man's march into space, it would be of interest to secure moving pictures of all of the important steps that were to be undertaken. By giving the movie industry the right to disseminate the film, that industry might finance our projects. I therefore approached one of the well known Hollywood authors and script writers, my old friend Mr. Michael Blankfort. He went at it enthusiastically, but unfortunately without success, as he relates in the following note which he kindly wrote last year for my use in the present article:

A Note on a Proposal to Finance a Moon Project

> Sometime in the early Fifties (1953-1955), Professor Fritz Zwicky visited my home and during the course of the evening mentioned the problem of financing a project of propelling pellets to the moon via rockets. There was to be a number of special cameras placed strategically around the world to record the launching and landing of the pellets. It was estimated that $100,000 would be sufficient to carry the experiment through. I volunteered to speak to Mr. Darryl F. Zanuck, at that time chief of production of 20th Century-Fox Films. I would offer his company the world film rights to the moon project for that sum. I saw Mr. Zanuck the next day and explained what was proposed and what was being done. He seemed not only interested but enthusiastic, and promised to take the matter up at the next meeting of the Board of Directors of his company. I mentioned that there was an urgency about the passing of time, and he replied that he would not wait but telephone. I saw him about forty-eight hours later. He reported that New York had rejected the proposal as outside their common run of business. His disappointment was almost as great as mine, but neither compared to Dr. Zwicky's.
>
> Michael Blankfort
>
> April 13, 1971

Next, my old friend Sydney Chapman stepped in. He had been in my audience at the University of Oxford in the spring of 1948 when I gave my Halley Lecture on Morphological Astronomy.[4, 9] He had been impressed with the power of the Morphological Approach, and considered organizing all of the projects of the International Geophysical Year (IGY) using this method and, as a special case, include experiments with artificial meteors. As a preliminary he told me that during a stay in Australia he would try to arrange for me to launch my meteors to the moon with rockets from the rocket base at Woomera. He was successful in interesting the cognizant agencies in Australia in my project. As I made preparations to go to Australia, a curious thing happened. On a beautiful Sunday afternoon in Pasadena I was called by the London Times. The reporter said that the Prime Minister of Australia, Mr. Menzies, had called Winston Churchill, inquiring what this crazy project of shooting the Moon from Australia was all about. Mr. Churchill, being quite as uninformed as Mr. Menzies, called the experts of the London Times, who also knew of nothing and who called me. It turned out that neither Sydney Chapman nor the Australian rocket men had ever thought it necessary to talk to the Prime Minister about my simple project. But that is where they had made their fatal mistake. Mr. Menzies became incensed, claimed that my project would give rise to world-wide repercussions, and he ordered the flight cancelled.

THE FIRST SHOT INTO THE UNIVERSE

I still had many far-sighted friends in the US Air Force. I had come to know these men after my tour in Germany as their technical representative, and after serving for many years on the Air Force Scientific Advisory Board under Generals Arnold, Spaatz, and Vandenberg. In particular, Dr. Knox Milsaps, Chief Scientist at the Alamogordo Air Force Base, and Dr. Maurice Dubin promised to watch for any space available in one of the research sounding rockets that might allow us to install our shaped charges. In August 1957 these two gentlemen informed me that an Aerobee rocket would be fired in October, and that about one cubic foot of space would be available for us in the instrument head. Mr. Cuneo and I therefore feverishly went to work on our shaped charges and the construction of timers that could be set to ignite the charges at the proper moment of the rocket flight.

All of our devices were ground tested at Alamogordo on the night of October 15, 1957. Next day some trouble arose, because another group of scientists had cosmic ray recorders installed in the rocket. They feared that our exploding charges would destroy their instruments. How we resolved this very difficult problem has been described elsewhere.[3, 10] In any case, the charges were ignited properly as we had planned at a height of 85 kilometers, exactly 91 seconds after the rocket had left the ground.

One very luminous pellet consisting mostly of TiC and some Al_2O_3 was ejected with a velocity of 15 km/sec, considerably in the excess of the escape velocity of the Earth. This pellet became the first man-made object to be shot into interplanetary space, and thus the first tiny artificial satellite of the Sun. Its luminous trajectory on the way out was photographed by many cameras, including the Palomar 48-inch Schmidt telescope located over 1000 kilometers to the West. The velocity of the pellet was determined by the Superschmidt telescope on Sacramento Peak in New Mexico, by means of interrupting the photographed trajectory with a propeller shutter.

PHASE 3

Unfortunately, both the scientific community and the world at large did not hear about our success until a few months later, because the Air Force would not release any data until they had made sure of all of the facts. Furthermore, everybody was excited because the Soviets already had successfully launched Sputnik 1 twelve days before. During the subsequent rush, when the United States tried to catch up with the Soviets, artificial meteor projects were forgotten and all of my plans to shoot chemically reducing projectiles into the Moon came to naught. Thus, to this very day we have no answer yet to the vital question of whether or not deeper lying moon rocks might contain water of crystallization that would make possible the installation of simple life-sustaining devices (solar furnaces, plus magnetohydrodynamic generators, plus electrolytic cells for the production of various elements), such as I had proposed and partly constructed twenty years ago.[3, 6, 7]

RECONSTRUCTION OF THE PLANETARY SYSTEM

With the launch of Sputnik 1, some modest phases of the plan,[+] that I had first outlined in my Halley Lectore[4] at Oxford in 1948, appeared within practical grasp. To prevent the various governments and their respective agencies from blundering into outer space and bungling things, as has been done throughout history with all large-scale new developments on Earth, some far-sighted men (and in particular the lawyers, physicians, pure scientists and engineers of the International Academy of Astronautics [IAA]) emphasized the necessity of international agreements to precede any practical constructive or destructive actions on extraterrestrial bodies and in outer space. These efforts led to the formulation of a preliminary Treaty for Outer Space which was signed by some eighty nations in 1968.

Many details still remain to be clarified and the members of the IAA and of the International Institute of Space Law are hard at work to analyse and formulate them. Once the basic aspects of the March into Outer Space have been isolated, analysed, and evaluated, some international organization such as the UN will have to decide which of the following three generic types of enterprises should be allowed and undertaken:

1. Scientific exploration and construction of research stations in space and on the various bodies of the solar system.
2. Exploitation of some of the extraterrestrial resources for use in pure and applied science, basic technology and medicine.
3. Exploration on a moderate, or large, scale for commercial and technical uses, including the establishment of general human settlements on the various bodies of a suitably reconstructed planetary system.[4]

[+] Zwicky's plan called for modifying the physical conditions of some of the presently uninhabitabitable bodies in the solar system; e.g., First, constructing subsurface installations on the Moon and the planetoids; second, making them habitable by changing surface conditions of the Moon to provide a breathable atmosphere of molecules heavy enough not to escape; third, reconstructing various large bodies of the solar system by moving them to other locations--Ed.

SOME MODEST AUXILIARY RESEARCHES
FOR THOSE WHO FOLLOW

For the past several decades funds for research have been rather overabundant, not always to the benefit of fundamental research. These funds have now become scarce, and it is useful to remind the young scientist in particular that very important research still can be done simply and cheaply. By way of illustration, here are a few proposals and investigations that, in part, existed even before the later rapid developments in space research.

First, I would mention a class of extraterrestrial bodies with which we can actually experiment because they come to us: meteors and meteorites. Although a considerable number of spectra of meteors have been observed, far more could be done through the systematic use of powerful Schmidt telescopes equipped with prisms and full size objective transmission gratings, some of which have now been successfully built to sizes of 45 cm x 45 cm, either as mosaics or ruled full size by myself, R. W. Wood, and John Strong and associates.[3, 11, 12]

More important, perhaps, many meteors actually come crashing down to the Earth's surface to lodge with us as meteorites. Since they have been traveling in interplanetary space for many millions, and probably even billions of years, they have been subjected to all sorts of extraterrestrial interactions and events. Therefore, in some respects, they may be considered as substitutes for artificial test bodies which we might otherwise launch intentionally into outer space. During their travel they have been exposed to cosmic rays, to the solar wind and electromagnetic radiations covering the whole wave length range from radio waves to Gamma rays, leaving the meteorites with the following tell-tale signs to be searched for:

a. **Excitation of** long duration pseudostable states that are analogous in character to the latent photographic images caused by sufficiently energetic quanta of light in heteropolar crystals, for instance. The energy thus stored in the solid meteorites can be determined by baking them out, as was done extensively by F. G. Houtermans and his collaborators at the University of Berne.

b. Protons from the solar wind will be imbedded to a depth of a few hundred Angstoms in the meteors, as they have been found in lunar rocks recently. Whether or not these proton layers in meteors survive the dive through the atmosphere and have ever been found in meteorites is not known to me.

c. Heavy nucleons of the cosmic rays leave permanent tracks in certain crystals and should therefore also be found in meteorites.

d. Finally, we know that fast solid particles ejected **from** solid inserts in shaped charges at velocities of up to 15 km/sec[3] can penetrate solid blocks, of steel for instance, to depths of one meter. There is a good chance that during their long travel large meteors will have been hit by small ones traveling at velocies even higher than 15 km/sec and should therefore occasionally exhibit straight long thin channel holes. Searching through various museums, I think that I may have spotted such holes, but the respective museum directors were reluctant to let me cut up their beautiful large specimens in order to test my expectation. Continuation of this type of inquiry might, however, some day be rewarding.[+]

e. Finally, the faint suspicion remains that the missing planet between Mars and Jupiter may have been blown apart by a nuclear fusion reaction, initiated for instance through the impact on it by a very fast body of "local origin," such as a nuclear goblin ejected from the sun or some interstellar ultrafast missile.[13] We shall be able to draw more definite conclusions once we have investigated whether or not some craters on the Moon have been caused by impacts capable of igniting nuclear fusion reactions and by actual experiments of our own with ultrafast particles.

Second, the results of some experimentation with extraterrestrial bodies can be observed from the Earth, inasmuch as nature itself provides the means. For instance, the Sun "experiments" with the Moon by illuminating it in various phases. The resulting successive heating and cooling of various parts of the Moon's surface could thus be observed and its heat conductive properties be derived. Sunlight falling on the Earth is partly scattered and can be observed illuminating the dark part of the Moon which faces us. Proper measurement would allow us thus to determine the Albedo of both the Moon and the Earth, the latter varying because of different cloud cover.

Third, direct measurements of the Albedo of the Moon were made in the 1940s by sending radar signals to it and observing the echoes, while recently the same was accomplished with Laser beams. These various attempts actually represent modest examples of passive experimental astronomy.

APPENDIX

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(can be obtained only from the Bookstore of the California Institute of Technology, Pasadena, Calif. 91109).

[+] I have also long considered the problem, during a concentrated meteor shower, of intercepting some of the fast incoming missiles by placing convenient targets in the stratosphere or ionosphere in order to test the meteors penetrating power. Since this will, however, soon be done more conveniently on the Moon, I have abandoned wracking my brains further on the matter.

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AMERICA'S FIRST LONG-RANGE-MISSILE AND SPACE EXPLORATION PROGRAM: THE ORDCIT PROJECT OF THE JET PROPULSION LABORATORY, 1943-1946: A MEMOIR[+]

Frank J. Malina (USA)[++]

I. INTRODUCTION

The years covered by this memoir, contrasted to those discussed in my first two memoirs,[1,2] were extremely hectic. It is very difficult for me even now to draw anything like a clear and coherent picture of them. Nevertheless, by drawing attention to unpublished material available in the archives of the Jet Propulsion Laboratory, I hope what I have to say will be of use to historians of astronautics.

Between 1943 and 1947 I became increasingly involved as an administrator of research. The Air Corps Jet Propulsion Research Project, GALCIT,[2] which numbered around 85 persons in 1943, grew to around 400 by 1946, and the amount of money to worry about increased from hundreds of thousands to millions of dollars annually. Although directly acquainted with all that was taking place on the Project up to 1944, both as regards ideas and their execution, by 1946 I was aware of more and more research activities but in less and less detail, and little of my time was free for carrying out research of my own—a situation unpleasing to one of my temperament.

Theodore von Kármán's connection with the California Institute of Technology (Caltech) became increasingly tenuous in 1942, and in 1944 he became absorbed with activities in Washington, D.C.,[3] where he took up residence. Consequently, in 1944 I took over contract negotiation from both technical and management points of view, tasks that required frequent trips to Wright Field at Dayton, Ohio, to Washington, D.C., and to other places. Life became a "between trips" kind of existence.

[+] Presented at the Fifth History of Astronautics Symposium of the International Academy of Astronautics, Brussels, Belgium, September 1971.

[++] Co-Founder and Director (1944-1946) of the Jet Propulsion Laboratory, California Institute of Technology. Trustee-Past President, International Academy of Astronautics.

By 1944 it was fairly evident that World War II would end in the defeat of the forces of fascism in Germany, Italy and Japan. But what then? In 1945 the harnessing of atomic energy for destructive purposes was demonstrated at Alamogordo, New Mexico, and then at Hiroshima and Nagasaki, Japan. As I was drawn into the councils of those with military responsibilities, I participated more and more in discussions of what should be done in the next war with long-range rocket missiles. Obviously, if atom bombs could be made light enough, they could be used as missile warheads. Such deliberations became more and more distasteful to me as the months went by. I had long been convinced that war between or by states with advanced technology was a form of national insanity, even before a way to release nuclear energy had been found. It seemed to me that ideas and effort were really needed now to find ways for "sovereign" states to function in peace together, rather than to develop better means of destroying themselves.

By 1946 I was mentally and physically exhausted. General Eisenhower is said to have remarked when the war ended that all he wanted to do was to go fishing; I felt the same way, except that I do not care very much for fishing. I had completed 10 years of rocket research, and dealt with problems on the fringes of basic and engineering science knowledge, devices requiring the use of explosives and toxic chemicals, the safety of our staff and of aircraft test pilots, frustrations resulting from dealing with administrators who had no grasp of the nature of research, travel by train and by air to meetings that frequently were not really necessary, etc. Thus, at the age of 34, I determined to make a serious appraisal of myself and of my hopes for the future.

When we had begun rocket research at Caltech in 1936, most of our original group of six was dedicated to the peaceful uses of rocket propulsion.[1] The design of a sounding rocket had been our first goal. Though I never lost sight of our first goal, world developments by 1938 dictated our participation in the military application of rocket propulsion. When the WAC Corporal became the first successful sounding rocket to exceed heights attainable by any other means in 1945, I felt a sense of personal fulfillment. I understood that this was but the first probe into extraterrestrial space, and that voyages to the Moon and planets would follow, but I also knew that there were now many others who would carry on the work necessary to reach these more distant goals. In 1936, the number of engineers in the world seriously interested in astronautics was probably less than 50; by 1946 there were several hundreds.

What troubled me most about leaving JPL was the separation from the members of the staff, many of whom were my closest friends and with whom I had shared many good and many trying times. I have never again worked with a group that was as cooperative and enthusiastic. But the new goal that I had set for myself was international cooperation. Although I left Caltech with a two-year leave of absence, which was renewed in 1949, I then allowed it to lapse and, in one way or another, devoted myself to international cooperation during the past 25 years. The night before I departed from Texas for Unesco in

Paris, one last effort was made to dissuade me from leaving the Jet Propulsion Laboratory (JPL). A general officer of the Ordnance Department telephoned me from Washington, D.C., and urged me to reconsider my decision. When I asked von Kármán for his advice he had told me that if he were younger, he would follow a path similar to the one I had chosen; it was unlikely that anyone else would get me to change my mind.

I made a tour of the East Coast to discuss Unesco with many persons before going to Paris. Albert Einstein thought that major points of the Unesco program were definitely worthwhile. He said we must have courage to fight for real issues and not allow Unesco to become impotent, like the Commission for International Intellectual Cooperation of the League of Nations. Vannevar Bush said that scientists must work together to stop wars for good. He did not know very much about Unesco but supported it. Lyndon B. Johnson, then the Congressman from the district of my home in Texas, said he was not acquainted with Unesco intentions and that the United Nations was just a "baby."

Upon my arrival in Paris, Joseph Needham, Head of the Natural Science Section of Unesco, assigned me the task of studying ways to break down the barriers to the free movement of scientists and engineers between nations. It certainly was not true that I became a member of the Unesco secretariat as a rocket expert, as was stated in an article hostile to the organization in the Saturday Evening Post entitled "Julian Huxley's Zoo." Unesco did not come within a smell of rockets before the International Geophysical Year (IGY) in 1957.

While at Unesco, outside my official duties, I wrote a popular article entitled "Unmanned Rockets towards Space" in 1950, upon the invitation of Kenneth W. Gatland, who served as the editor of a collection of articles for a book called Rockets into Space. But that venture was given up by the publisher in 1954. In 1950, I also wrote "A Short History of Rocket Propulsion up to 1945" for the Princeton University series of volumes on Jet Propulsion and High Speed Aerodynamics of which, at that time, my long-time colleague, Martin Summerfield was editor. That article was finally published in the volume Jet Propulsion Engines in 1959.[4]

I did not resume work in astronautics until after the launching of the Sputnik by the Soviet Union in 1957, an event that made more evident than ever the need for international cooperation in this field. Andrew G. Haley, one of the founders with us of the Aerojet-General Corporation[2] and then President of the International Astronautical Federation, and von Kármán, who had become active in Federation affairs, told me that it was all very well to work quietly as an artist in a Paris studio (which I had been doing since 1953 after leaving Unesco) but that my experience in astronautics was wasted. They urged me to participate in the work of the Federation.[3] After much discussion with my wife, I decided to accept appointment as a representative of the Federation to Unesco. By 1959, I was again devoting most of my time to astronautics in the Federation, especially in connection with the establishment and direction of the International Academy of Astronautics.

But to return to the subject of this memoir . . . .

II. THE SITUATION IN 1943

The Air Corps Project at the beginning of 1943 was consolidating the successfull development of solid-and storable-liquid-propellant engines for aircraft superperformance applications with research directed to improving the propellants, raising engine performance, and increasing their thrust and duration. Cooperation on matters of development and production was maintained with the Aerojet Engineering Corporation for those programs sponsored by the Air Corps and the Navy.[2] The Project had placed solid-and storable-liquid-propellant rocket engine design on a sound scientific foundation. Practical information had been provided engineers, permitting the design of uncooled motors to meet specifications for thrusts of up to around two tons for durations of up to about 75 seconds. By the end of World War II, information on the design of cooled liquid-propellant engines and of pumps was well advanced.

Albert A. Christman, in his history of the Naval Weapons Center entitled Sailors, Scientists and Rockets, observed: "A suggestion by a Navy captain that it would be desirable to have a cooperative effort between Goddard and Malina brought out Goddard's view that the work in Pasadena was about the stage in 1940 that his work had been in 1925. He referred to the Caltech program as the 'Student Work.'"[5] Goddard evidently did not subscribe to von Kármán's maxim: "It is always wise to remember that someone else might be just as clever as oneself." Goddard's opinion of our efforts does not surprise me,[1] but he must have been surprised when, within two years after his remarks we had successfully developed and put into production for the Air Forces and the Navy service-type solid-and storable-liquid-propellant engines. These became, respectively, the progenitors of the engines in, for example, the Sergeant, Polaris, and Minuteman missiles, and the Tital missile and the Apollo Command Module and Apollo Lunar Excursion Module.

This raises an interesting question concerning developments in Britain, USA and USSR after the end of the war, a question that should be probed by historians of rocketry and astronautics. I believe a good case can be made to show that military obsession in these countries for continuing certain developments of German rocket technology caused a vast waste of funds. The obsession gave priority to rocket engines using liquid oxygen (LOX), as in the case of the V-2. In the U.S.A., though the development of engines using a composite solid-propellant and a storable-liquid-propellant combination was not dropped, it certainly was assigned a lower priority. But today, not a single American military operational missile uses a LOX engine, instead they are propelled by descendants of solid-and liquid-propellant engines developed at JPL before the end of World War II.

The investment in LOX engines turned out to be an overall technological gain, for they were indeed needed for extraterrestrial space activities, but they were not then and are not now of primary interest to the military services. What is more, solid and storable liquid propellants also have an important role to play in the propulsion of spacecraft at this phase of the "space age." Nonetheless, popular opinion, even the opinion of some who should know better, has held that rocket developments in the U.S.A. lagged far behind that of Nazi Germany. That belief is patently false, but myths die hard.

III. ORIGINS OF THE ORDCIT PROJECT

At the Air Corps Project in 1943 we continued to follow the directives of 1939 that limited us to rocket engines for use with aircraft.[2] In the Summer of 1943 this situation changed radically. In early July, von Kármán received a request from the Commanding General of the AAF Materiel Center, Wright Field. He was asked to study and comment on three British Intelligence reports on reaction propulsion devices for projectiles and aircraft supposedly being developed in Germany.[6,7,8] Von Kármán's comments, based on an investigation that he, Hsueh-Shen Tsien (Chien Hsueh-Sen) and I made, were sent to Wright Field on August 2. Although much of the data from German prisoners in the reports was incorrect, inexact, and exaggerated, it was possible to draw some interesting conclusions. The fact that our conclusions bore little resemblance to actual German missile and aircraft developments, as we learned later, is irrelevant to their impact on the 1943 military scene in the U.S.A. Fascinating background material on these intelligence reports, to be read with circumspection, can be found in the book The Mare's Nest by David Irving.[9]

The AAF Liaison Officer at Caltech at this time was Col. W. H. Joiner, a most congenial and helpful officer. He immediately appreciated the significance of our conclusions and suggested to me that a study should be made of the possibility of propelling ballistic missiles with the rocket engines we had developed or that were available at Aerojet. I turned to Tsien for help, and the two of us completed our study in November.[10] The results showed that although ranges in excess of 100 miles could not be reached with available engines, rocket missiles could be constructed that had a greater range and a much larger explosive load than rocket projectiles then being used by the Armed Forces. After discussing the analysis with us, von Kármán decided to attach a memorandum to our report proposing that a development program be initiated along the lines we had indicated.[11] These documents for the first time carried the name "Jet Propulsion Laboratory." Joiner sent the memorandum and analysis to the Commanding General at the AAF Material Center. Captain R. B. Staver, an Army Ordnance Liaison Officer at Caltech, occupied an office next door to Joiner. Staver also forwarded these same documents to Colonel G. W. Trichel, Chief of Rocket Development Branch of the Army Ordnance Department.[2]

Von Kármán, Tsien and I at this point concentrated our thoughts on the technical problems of long-range missiles and on what appeared to us to be the most reasonable steps to be taken to develop them on the basis of current experience in the U.S.A. with solid and liquid propellant rocket engines. Staver and Joiner, on the other hand, pursued quite a different thought process. Staver later told me that they were concerned with assuring continued support of the development of rocket and other types of jet engines and of their application by the military services after World War II. They feared the historic tendency of the government to drop potentially important research for military purposes when a war ended. Furthermore, they believed that our Project should not only be continued, but expanded to become a center of jet propulsion and missile research and development.[13] I have pointed out before that there was a conflict of opinion as to the kind of work that could or should be undertaken by an academic institution of higher education and research.[2] Von Kármán and I were of the view that Caltech was an appropriate organization to take responsibility for basic engineering research, but not for the development of prototypes of engines and of vehicles that involved problems of meeting production and end-result specifications. The fact that the ORDCIT Project did include these latter activities can be understood only in terms of the special situation prevailing in the U.S.A. in 1944.

The response of the military services to the two documents on the development of long-range missiles planted a seed for the bitter inter-service military rivalry that took place in the 1950s. I recall discussions with officers in the Air Forces and the Ordnance Department on the appropriate "botanical" classification of a rocket missile. Those concerned with army ordnance said that, since long-range guided missiles followed a ballistic trajectory like a gun projectile, such missiles were clearly a responsibility of the Ordnance Department. Those responsible for long-range aircraft bombers said that, since a long-range guided missile needed aerodynamic control during the first phase of flight in the atmosphere, the Air Forces clearly should be responsible for their development, and they called the missiles "pilotless aircraft."

But the Air Forces did not respond to our proposal, much to our amazement. Instead, von Kármán received a letter on January 15, 1944, from Trichel of Army Ordnance. It expressed not only interest in the proposed program, but a desire that Caltech undertake a more intensive program than originally outlined.[12] Trichel urged that a revised and more inclusive program be undertaken at the earliest possible date. He further stipulated that the Ordnance Department was prepared to furnish the necessary funds to cover such a project providing Caltech, in turn, was willing to give the necessary emphasis to the undertaking in the assignment of personnel and facilities. He also recommended that a proposal be submitted that would include a chronological schedule of the studies to be made, models built, etc.; further, if such a project was decided upon, it would be advisable to make a contract with Caltech on a cost-plus-a-fixed-fee basis. The plan of operations should initially cover not more than one year and the expenditures should not exceed $3,000,000 for the one-year program.

Trichel's letter threw us into a proper dither! We prepared a new proposal incorporating his suggestions, and von Kármán, with the support of Robert A. Millikan, Chairman of Caltech's Executive Council, obtained the approval of the Caltech Trustees to put forward the proposal to the Ordnance Department. On January 20, Brig. General B. W. Chidlaw, Chief, Material Division Office, Assistant Chief of Air Staff, directed a letter to the Commanding General of Materiel Command, Wright Field, requesting the following information:[12]

a) Will the Army Air Forces authorize the use of the facilities at GALCIT by the Ordnance Department?

b) Will such a long-range development contemplated by the Ordnance Department conflict with work being conducted by the Army Air Forces?

Von Kármán also responded on February 1 to Major General Frank O. Carroll, Chief Engineering Division, AAF Materiel Command, Wright Field, inquiring about AAF interest in research directed towards the development of aircraft or pilotless-aircraft traveling at transonic speed, and apparently helped the Materiel Command formulate answers to the above questions.[12,14] Accordingly, on February 17 the AAF Material Command at Wright Field cleared the Ordnance project to proceed at GALCIT, so far as the AAF was concerned. Also, Carroll sent a letter to von Kármán approving, under his supervision, a project for "athodyd" (ramjet) engine development with the Ordnance Department that could be used on aircraft, provided AAF research would not be retarded in any way.[12]

Events now moved rapidly. On February 28, 1944, von Kármán submitted on behalf of Caltech a new proposal, based upon Trichel's suggestions, to Major General G. M. Barnes, Chief of the Technical Division, Ordnance Department, in Washington, D.C. This proposal was accepted practically intact[12] (It appears in the appendix). A Letter of Intent for the Army Ordnance program was placed with Caltech on June 22 "for services consisting of research, investigation and engineering in connection with the development of long-range rocket missile and launching equipment and for complete reports, drawings and specifications describing all work done in connection therewith." An expenditure not exceeding $1,600,000 was authorized. A definitive contract followed, and entered into force on January 16, 1945, with the following objectives:[12]

a) The missile would have a minimum weight of high-explosure payload of 1000 pounds.

b) Maximum weight of the missile would not exceed a weight consistent with good design and maximum payload.

c) The missile would have a range of up to 150 miles.

d) Target dispersion at maximum range would not exceed 2 percent for a missile suitable for direction by remote control.

e) The velocity would be sufficient to afford protection from fighter aircraft. The termination date of this contract was set at December 22, 1954; however, it was later extended through June 30, 1946; the total funds provided amounted to $3,600,000.

This expanded program led to a reorganization of the Air Corps Jet Propulsion Research Project, GALCIT, into the Jet Propulsion Laboratory, GALCIT. The new program was given the designation ORDCIT Project (ORDCIT is an acronym for Ordnance-California Institute of Technology).

IV. REORGANIZATION OF THE AIR CORPS PROJECT INTO THE JET PROPULSION LABORATORY

The ORDCIT Project required rapid expansion of the staff and facilities of the Jet Propulsion Laboratory (GALCIT), or, JPL. During the period under consideration, JPL was attached to the Guggenheim Aeronautical Laboratory (GALCIT) directed by von Kármán. He remained titular director of GALCIT until 1949, when he became Professor Emeritus and Clark B. Millikan succeeded him. By this time, JPL had been separated from GALCIT and came directly under the overall administration of Caltech.

While we were in the midst of preparing plans for carrying out the program of the ORDCIT Project, von Kármán underwent serious abdominal surgery at the end of May 1944, in New York City, which prevented him from returning to Pasadena until September. While he was recuperating in New York, General H. H. Arnold, Commanding General of the AAF, asked him to undertake the creation of the Scientific Advisory Board to the Chief of Staff of the AAF "to investigate all possibilities and desirabilities for postwar and future war's development as respects the AAF." Von Kármán left for Washington, D.C., in December 1944, thereafter returning to Caltech only for short period of time.

The scope of the ORDCIT Project posed Caltech administrators with novel problems. How would they manage the range of activities, the size of the JPL staff, and the amount of money involved? They decided to establish a JPL Executive Board, responsible to the Caltech administration, whose task would be to oversee the general policies of JPL administration and the implications of any new technical developments that took place. Then, when von Kármán took leave of Caltech, the question of who would direct JPL had to be resolved. C. B. Millikan was asked to become Chairman of the JPL Executive Board, and I took over as Acting Director at JPL. But the implications of this division of responsibilities soon required clarification.

From my long experience of working with von Kármán, I knew that a certain coolness existed between him and C. B. Millikan; to be sure, I had inherited this feeling.

I also could not easily forget that, if von Kármán had not overruled him, Millikan would have stopped the GALCIT Rocket Research Group in the spring of 1936. When Millikan stated in the autumn of 1944 that he expected as Chairman of the JPL Executive Board to chair regular weekly JPL project conferences, I exploded. I told him that I could not continue making technical and administrative decisions if the conferences were run by someone who was not intimately aware of what was going on in the laboratory from day to day. Von Kármán supported me.

Neither my relations with the JPL Executive Board, nor those of Louis G. Dunn, who succeeded me as JPL Director in 1947, were ever very satisfactory. On one occasion in 1945 I became so irritated with Millikan when he presented a summary of work underway to the Board, that I slammed down my papers, announced my resignation, and stalked out of the meeting. The next day, after taking into account the reasons for my displeasure, Millikan and other Board members convinced me to change my mind. But the problem of divided responsibility was never resolved. Dunn, when he became JPL Director after I left, simply refused to attend meetings of the JPL Executive Board, and Caltech finally dissolved the Board in 1948.

In 1944, Col. L. A. Skinner was designated Liaison Officer for the Army Ordnance Department. Those of us concerned with solid propellant rocket engines were acquainted with his studies in the 1930s of nitroglycerine-nitrocellulose as a propellant. I also had met him when he worked with the "Indian Head Group" at Indian Head, Maryland, in the early 1940s.[5] Joiner was succeeded by Col. E. H. Eddy as Liaison Officer for the AAF, and Lt. Col. J. W. Newman was designated Liaison Officer by the Army Ground Forces.

Beginning in 1944, research at JPL was carried out on four major projects: JPL-1 (Project MX 121 of the Aircraft Laboratory of the AAF Materiel Command, was a continuation of the program previously carried out for the Aircraft Laboratory[2]); JPL-2 (Project MX 363 of the Armament Laboratory of the AAF Materiel Command, begun in 1943,[2] was on hydrobomb research); JPL-3 (Project MX 527 of the Power Plant Laboratory of the AAF Materiel Command, begun in 1944, was primarily on ramjet engine research[2]); and JPL-4 (the ORDCIT Project).

The ORDCIT Project involved not only fundamental engineering research on thermal jet propulsion engines, propellants, and the design of guided missiles, but also the construction of missiles and launching devices for firing tests. The firing tests were to be made in cooperation with the Ordnance Department. Thus, we had to become accustomed to thinking in terms of design and construction of much larger devices and equipment than before. We were greatly helped in meeting this situation by Romeo R. Martel, Caltech Professor of Civil Engineering and a member of the JPL Executive Board, and by Aladar Hollander of the Byron Jackson Co., of Los Angeles, a manufacturer of pumps. The design of large constructions required to develop and launch missiles was guided by William A. Sandberg of the Consolidated Steel Co. of Los Angeles. Mark Serrurier of Caltech supervised the design of installations for ramjet engine studies. Eugene M. Pierce, Sr., an architect who had been my personal assistant since 1943, and W. Hertenstein, head of maintenance and buildings at Caltech, bore the brunt of designing and contracting for new buildings that had to be constructed as quickly as possible. The construction program was further aided by the Ordnance Liaison Officers--Skinner up to August 1945 and thereafter, Colonel Benjamin S. Mesick. Val C. Larsen took charge of Laboratory administration in 1946, and supervised material procurement. William R. Stott, Assistant Comptroller at Caltech and I spent many days on contract negotiation. The layout of buildings and special facilities of JPL in June 1945 is shown in Figure 1.

Fig. 1
Layout of the Jet Propulsion
Laboratory. June 1944

The organization chart of JPL as of January 9, 1945, is shown in Figure 2. The Technical Section Chiefs were:

H. J. Stewart, Section 1, Research Analysis
J. V. Charyk, Section 2, Underwater Propulsion
H. S. Seifert, Section 3, Liquid Propellant Rocket Engines
C. Bartley, Section 4, Solid Propellant Rocket Engines
P. Duwez, Section 5, Materials
S. A. Johnson, Section 6, Propellants
W. B. Barry, Section 7, Engineering Design
J. Amneus, Section 8, Research Design

Fig. 2
Organization Chart of the Jet Propulsion
Laboratory, 14 August 1945

W. H. Pickering, Section 9, Remote Control
W. D. Rannie, Section 10, Ramjet Engines
S. J. Goldberg, Section 11, Field Testing

Tsien, who had been the first chief of the Research Analysis Section, left Caltech for the Massachusetts Institute of Technology in 1946. He returned in 1948 to set up the Jet Propulsion Center established at Caltech by the Daniel and Florence Guggenheim Foundation. Martin Summerfield returned to JPL from the Aerojet Engineering Corporation in the autumn of 1945 to take part in planning and research analysis of possible applications of rocket propulsion to extraterrestrial space flight.

I have already discussed from my viewpoint the high points of research undertaken on solid- and liquid-propellant rocket engines and on ramjet engines up to the end of 1946 at JPL in my memoir on the Air Corps Jet Propulsion Research Project.[2] I shall discuss briefly below the research on guidance and control of missiles that William H. Pickering, a later Director of JPL, initiated in 1944.

V. 1944 MISSION TO EUROPE

Colonel F. F. Reed, called "Froggy" by his friends, visited JPL in the summer of 1944. He was Assistant Military Attache, Ordnance Department, in London. We decided that I should return his visit by going to Great Britain to study rocket and ramjet engineering research, to obtain firsthand experience in the target area of the V-1's and V-2's, and to inspect the parts of the V-2 that had strayed off course and landed in Sweden. I went to Washington at the beginning of September for a briefing and to obtain orders for my visit from the European Theater of Operations (ETO). After some delay, and a return trip to Pasadena,[16] the orders appeared. During the first week of October I flew to Prestwick, Scotland, and then took the train to London. Since my trip included an inspection of German V-1 launching sites and other kinds of installations in northern France (Pas de Calais region) liberated in August, I was given the rank of Colonel (assimilated rank, in case of capture), with the instruction that I was not to wear an insignia on my uniform. Considering that I had received the rank of 1st Lieutenant in the Officer Reserve in 1942, which I had to resign in order to continue rocket research at Caltech, I considered the jump in rank pretty good.

Upon the urging of Reed, I designed and had a tailor make a shoulder patch with a rocket on it--perhaps the first U.S. military insignia signifying rocket missiles (Figure 3). Although 32 years old, I looked much younger, and on my return journey, when I checked in at Bolling Field in Washington, D.C. to catch a B-17 to Los Angeles, the sergeant at the desk looked at my identity card, then looked at me and said, "Don't kid me, you can't be a Colonel!"

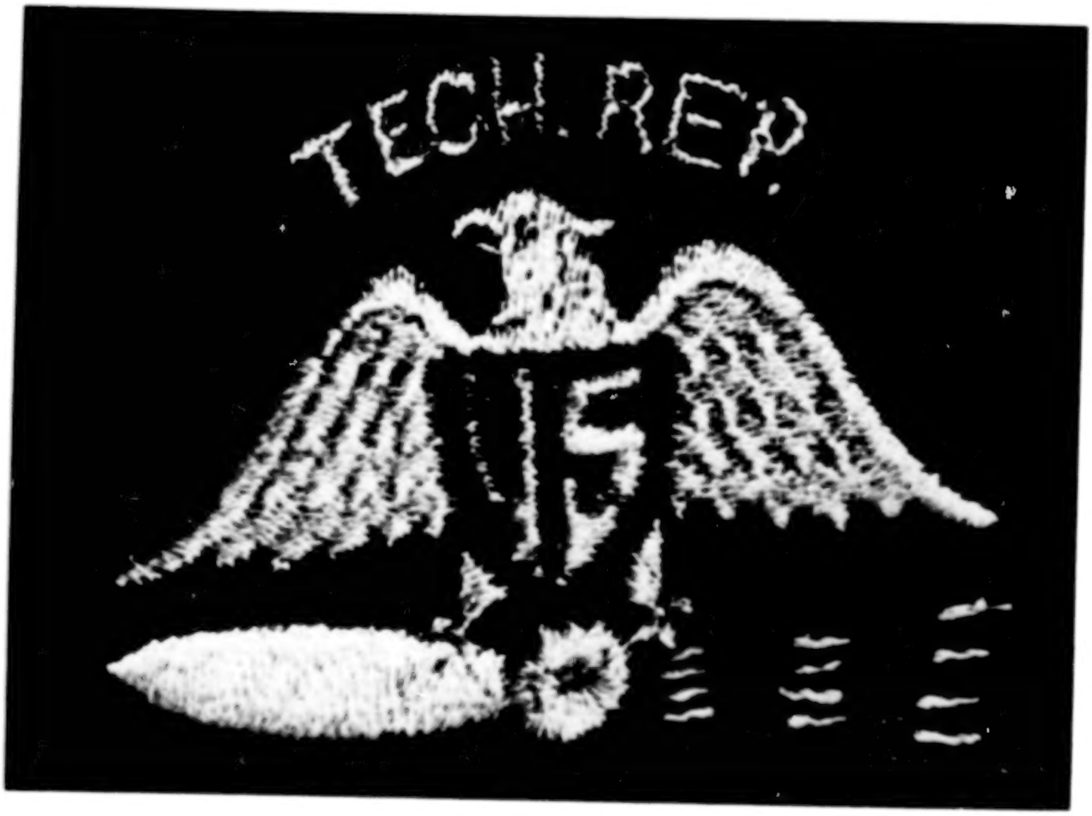


Fig. 3
Unofficial Rocket Missile Shoulder Patch Designed and Worn
By the Author in the European Theater of Operations, 1944

Eighty-two names of engineers and scientists in the United Kingdom appear on the list of those with whom I discussed various aspects of my mission, among them: Sir Alwyn D. Crow, Controller of Projectile Development, London; W. Blackman, Chief Superintendent, Projectile Development Center, Abeporth, Wales; J. E. Lennard-Jones, Chief Superintendent, Armament Research Department, Fort Halstead; Capt. A. Richards, Superintendent of Torpedo Exp. and Div. Greenock, Scotland; Commodore F. Whittle, Power Jets Ltd., Whetstone.

The discussions were quite open on both sides, although I knew that some information was held back, just as I held back on some of our plans and developments. Compared to research practice in the U.S.A., I was particularly impressed by the extent of British theoretical analysis and the length of debate on pros and cons before a decision was made to build something. This difference in approach was in large part due to the much more limited financial and manpower resources available in wartime Britain. I realize that our Private F fiasco, to be described below, might well have been avoided if a more thorough theoretical analysis of an unguided winged missile had been made.

The main interest in Britain in 1944 centered on the improvement, under the dominating personality of Sir Alwyn Crow, of an anti-aircraft rocket missile. Under his leadership, unguided anti-aircraft ballistite UP rockets (UP for Unrotated Projectile) were developed and used in the Battle of Britain in 1941. Some said that the UP's should have been called "misguided" missiles, for, as Charles C. Lauritsen, who watched them perform in London that year is quoted to have said: "I don't think they ever shot down a bomber...."(5, p. 108) They did make a lot of noise, which perhaps gave a psychological boost to the people.

By the time I arrived in London in October 1944, only a few V-1's were still arriving during the night, launched from aircraft over the North Sea because land launching sites on the other side of the English Channel had been captured by the Allies. V-2's bombarded London from bases off the coast of Holland. One shook-up a conference I was attending at Fort Holstead. My first experience of this sort left me rather disturbed, but my colleagues continued the meeting as though nothing had happened.

Information was available on the V-2 and on German engines using LOX and hydrogen peroxide (H_2O_2). The British were of the view that the nitric acid-aniline propellant combination was less suitable than hydrogen peroxide and, hopefully, nitromethane. We at JPL did not go overboard on either of these latter chemicals as the British did. (By 1944, we had good reasons to doubt the great expectations of John W. Parsons and Fritz Zwicky for nitromethane as a monopropellant. It was tested extensively both at JPL and at Aerojet and, as far as I know, nitromethane was finally given up as a rocket propellant because it is sensitive to shock and is difficult to use as a rocket motor coolant.) Test facilities for liquid-propellant engines were still in a very primitive state in Britain, and work was just getting underway on composite solid-propellant engines of long-burning duration.

There was much interest in the possibilities of the ramjet engine, mainly because of German claims, especially by Eugen Sänger. The British knew considerably more than we did at JPL about the problems that needed to be solved to make a ramjet a practical device.[2] Ramjet studies were being carried out in England at Power Jets, Ltd., under Lloyd and Constant. Here, also, Air Commodore Frank Whittle was continuing his development of turbojet engines, and he and I compared experiences on the vicissitudes of those who try to get unorthodox ideas accepted.

I inspected the parts of the V-2 brought from Sweden to the Royal Aircraft Establishment at Farnborough early in 1944. Design elements of the engine and the guidance system were still not completely understood. The latter was especially of interest to us at JPL, since we now confronted the problems of missile guidance and control.

Between November 16-26, 1944, I visited France with Captain C. E. Martinson, who had been assigned by Reed to look after me during my mission. Our flight across the English Channel in a Douglas Dakota (DC-3) aircraft provoked me to say that we should not have crossed so quickly to Paris. We landed in a mist at the end of the afternoon and got out of the aircraft. When a pilot appeared, we asked for our transport to Paris. He exclaimed: "Paris? The mailbag had Brussels on it so that's where you are!" (The following significant facts were uncovered by the superstitious to account for our plight: we were on flight No. 13, the Dakota's number added up to 13, and there were 13 passengers!) Once in France, we inspected V-1 launching sites at Wizerne, Montreuil and Sivacourt. Strategic bombing by Allied aircraft had produced only minor damage to the rocket sites, whereas nearby villages were a shambles. At Mimoyecques and Watten were very large constructions whose purpose up to that time no experts could make out.

Martinson and I celebrated Thanksgiving Day with American officers in Paris. After a turkey dinner at the Hotel Plaza Athenee, we were taken in hand by a group of bomb disposal officers who had five gallon bottles of something called "desert juice"--really, I think, a mixture of cognac and cointreau, which would have been better used as a rocket propellant. On the flight back to London the next morning, H. P. (Robie) Robertson of Caltech, who was with the O.S.R.D. Mission in London, poured coffee into me to combat its effects.

While crossing the Atlantic on my return journey during the second week of December, the many hours gave me a chance to think over the implications of all I observed on my tour. When I had departed from JPL in October, plans were far advanced for testing the first ORDCIT solid propellant missile, Private A, for the design of a winged version, the Private F, and for the liquid propellant Corporal. Turning over the many bits of information stored in my mind, I suddenly realized that the first objective our rocket research group had set for itself in 1936 was within reach--a sounding rocket.[17-19] We now had reliable solid- and liquid-propellant engines with which to build it. But first I had to sell the idea to the Ordnance Department.

Before proceeding on to Pasadena, I visited Trichel and his staff during a stopover in Washington, D.C. After reporting on my European mission, I presented my wish to have JPL design, construct and test a sounding rocket. I explained that the program could be carried out rapidly if modest requirements were set (to lift a 25-lb. payload to 100,000 ft). I also pointed out that a rocket with a liquid-propellant engine could be considered as a small-scale test version of the Corporal, experience would be gained with launching techniques, and the rocket might be considered a first step in the development of a guided anti-aircraft missile. The proposal was immediately received favorably. The Army Signal Corps established requirements for the rocket that met its meteorological payload needs.[2] By January 16, 1945, a study of the proposed sounding rocket, requested by the Ordnance Department, was completed by Stewart and me[20] (See Section VIII).

VI. THE PRIVATE A

The first missile to be tested used a solid-propellant rocket engine that could be quickly provided, as proposed by Tsien and the author in 1943.[10] It was called Private A, with the intention to name subsequent missiles in hierarchical order of army ranks. The JPL missile series ended in 1954 with the Sergeant, a solid-propellant surface-to-surface missile with an inertial guidance system. The Private A (also designated as the XF 10S1000-A) was designed to provide experimental data on the effect of sustained rocket thrust on a missile stabilized with fixed fins and on the use of booster rockets for missile launching.[21] A photograph of the missile is shown in Figure 4. It had a length of 92 in., a maximum diameter of 10.25 in. and 4 tail fins extending 12 in. from the body. The gross weight was 500-550 lb., including a payload of 60 lb. The solid-propellant motor, manufactured by the Aerojet Engineering Corp. (now Aerojet General Corp.) delivered a thrust of 1,000 lb. for about 30 sec. The specific impulse of the asphalt-base castable propellant GALCIT 61C was 186 sec.

The Private's launcher was a 36-ft.-long rectangular steel boom of the truss type, with four guide rails inside the truss. It was mounted on a steel base and both the lateral and vertical angles could be varied (Figure 5). The missile was boosted by 4 modified Ordnance Department aircraft armament rockets in a cluster as shown in Figure 6. The 4 rockets delivered a thrust of 22,000 lb. for 0.18 sec. They completed their burning and disconnected from the missile before it left the launcher. Full details on the design of the Private A, its booster and launcher, can be found in References 22 to 24.

Firing tests were made in the Mojave Desert at Leach Spring, Camp Irwin, near Barstow, California, between December 1-16, 1944, while I was in England. Twenty-four rounds were fired, with an average range of approximately 18,000 yards; the maximum range was 20,000 yards (11.3 miles). The missile reached an estimated peak height of 14,500 ft. and an estimated maximum speed of 1,300 ft./sec. A view of the smoke trail of a

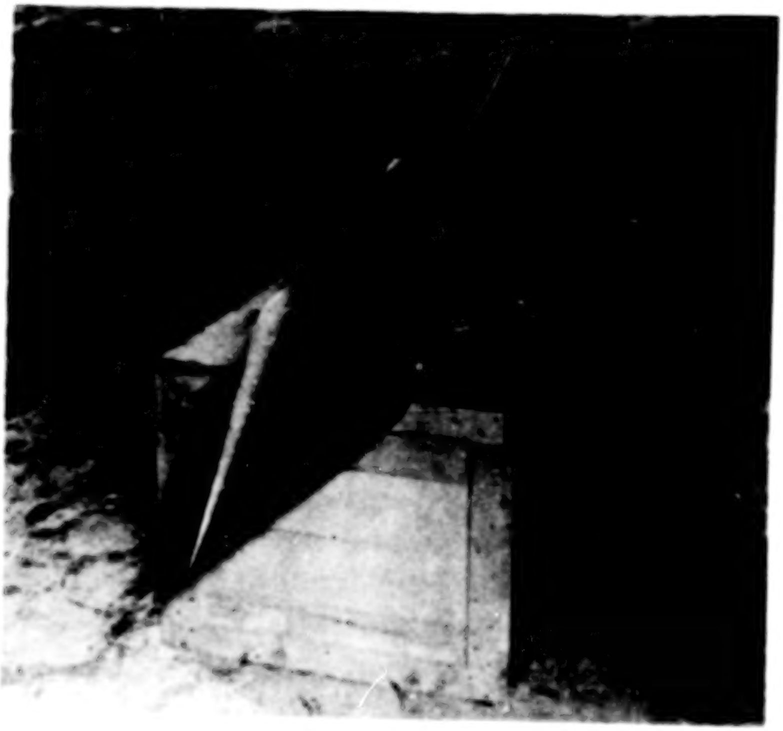


Fig. 4
View of the PRIVATE A Missile

Private A in flight is shown in Figure 7. Trajectory analyses were carried out by W. Z. Chien and C. C. Lin.[25,26] The firing tests were completely successful, meeting all of the objectives specified for the program. The Private A became the precursor of composite propellant rocket engine missiles the Sergeant, Polaris, Minuteman and Poseidon and of anti-missile missiles.

VII. THE PRIVATE F

Tsien and the author[10] also proposed the addition of wings to a missile having the characteristics of the Private A, estimating that the range would be increased by about 50 percent with a reduced payload. We pointed out that the problems of stability and control of such an unguided missile were very complicated. The winged Private A was designated Private F (also the XF 10S1000-B),[21] and used the same Aerojet solid propellant engine. The missile was provided with fixed wings, having a span of 5 ft., stubby wings of 3 ft. span were placed at the forward end for trimming the aerodynamic forces and, at the rear, horizontal stabilizers and a vertical fin (Figure 8). The same Private A booster rockets were used. A new launcher was constructed with two rails, in order to clear the wings and tail surfaces. Design details can be found in Reference 27.

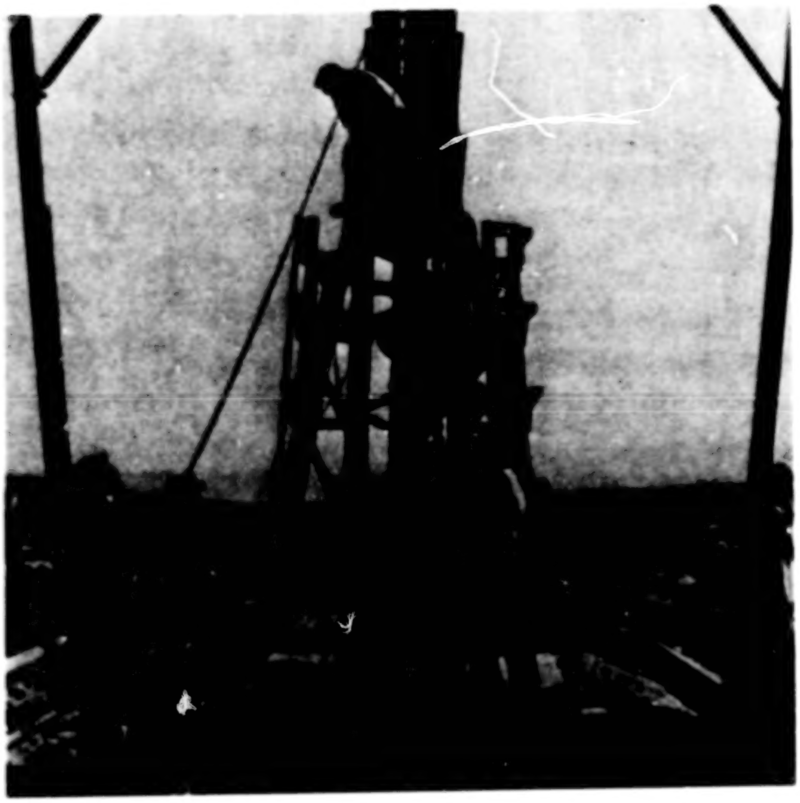


Fig. 5
View of the PRIVATE A Launcher

Firing tests were made at Hueco Range, Fort Bliss, Texas, between April 1-13, 1945. The tests began on April Fool's Day and turned out to be quite appropriate to the occasion. All of the Private F rounds, though successfully launched, went into a tail spin after a short flight (Figure 9). A striking corkscrew smoke trail was drawn in the sky by the rocket jet. Engineers concluded after a post mortem that better performance might have been obtained if the missile had been constructed with greater precision and if the lifting surfaces had been more readily adjustable.[28,29] In calmer times, after the war, when the use of funds is more carefully scrutinized, programs such as the Private F would have begun only after more theoretical studies of such a complex device had been made, and more care would have been taken in its construction.

While tests of the Private F were underway at Fort Bliss, Texas, we visited the nearby missile test range being prepared for the Ordnance Department at White Sands, New Mexico. Here, facilities were built for the tests of the WAC Corporal. These tests, in September, inaugurated the White Sands Proving Ground, then under the command of Col. H. R. Turner.

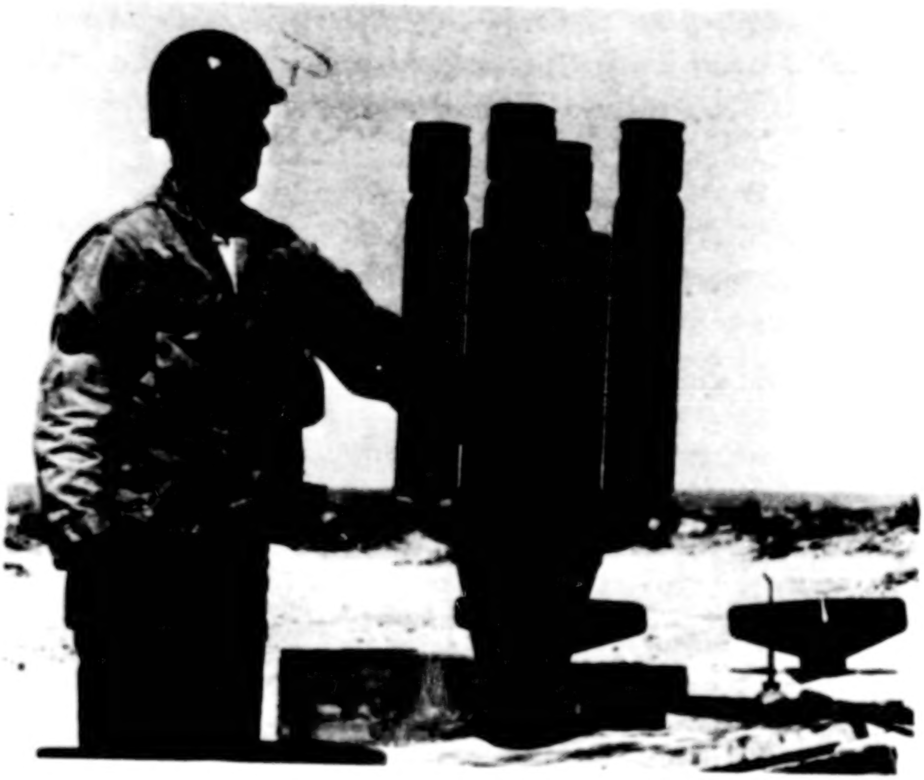


Fig. 6
Booster-Rocket Cluster for Launching the PRIVATE A Missile

VIII. THE WAC CORPORAL

The third long-range missile that Tsien and I considered[10] was to be propelled by a pressure-fed storable-liquid-propellant rocket engine of the type already developed at JPL. Our proposal contained the basic design ideas that were used in the WAC Corporal sounding rocket. Since a guidance and control system for missiles was unavailable in 1943, we proposed that a liquid-propellant missile be boosted out of a launcher by an unrestricted burning solid-propellant rocket. If launched at sufficient speed, tail fins would provide the necessary restoring force when the missile was disturbed into yaw by a cross-wind.

I have mentioned my brainstorm over the Atlantic in December 1944 that led me to obtain from the Ordnance Department authorization to design and construct a sounding rocket. It is appropriate here to review briefly the historical background of sounding rockets.[1,30] The possibility of employing rocket propulsion for lifting a vehicle to great heights was realized at the beginning of this century. Robert H. Goddard first gave serious consideration to this possibility in about 1914. At first he studied the

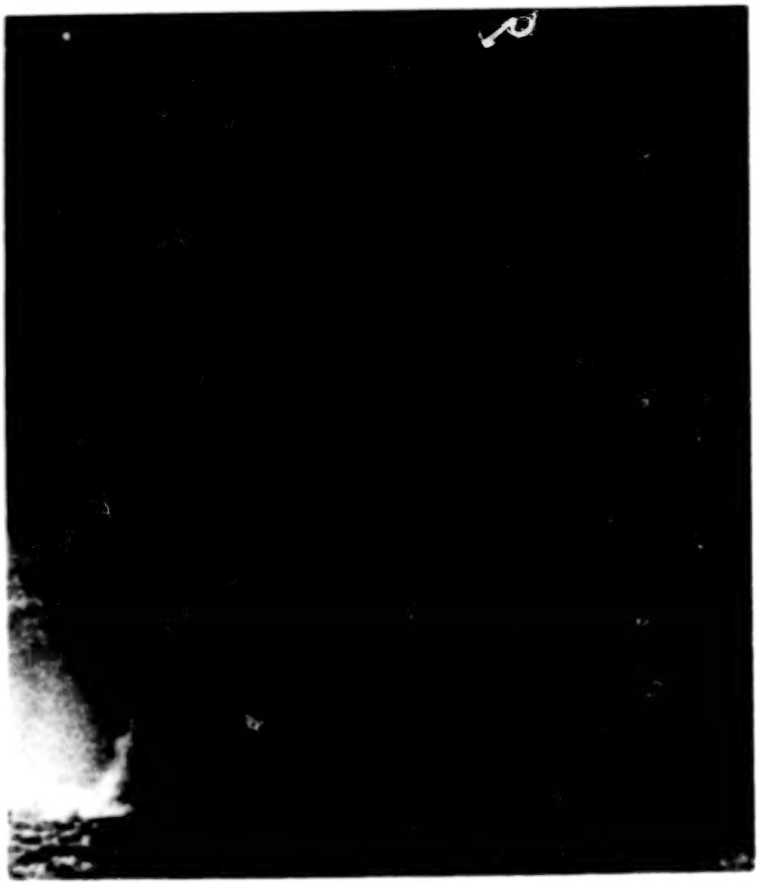


Fig. 7
View of the Smoke Trail of the PRIVATE A In Flight

feasibility of using constant-volume-process, short-duration smokeless powder rocket engines.[31,32] A more rigorous analysis of the flight performance of a rocket propelled by successive impulses was made by Tsien and the author in 1939.[19] A historical summary of sounding rockets is contained in a book recently released by NASA,[33] though it should be used with caution by historians.

Black powder rockets were capable of reaching several thousand feet before the 20th century, and balloons in the 1930s could reach altitutes of about 100,000 feet. A successful sounding rocket, therefore, had to surpass the altitudes achieved by balloons. Furthermore, to be useful, it had to be designed to minimize costs of production and of servicing and maintenance. In the 1920s Goddard had decided that a liquid-propellant rocket engine offered better possibilities for constructing such a sounding rocket, and he obtained limited financial support for developing one from the Smithsonian Institution.[34] The Daniel Guggenheim Fund for the Promotion of Aeronautics, upon the urging of Charles A. Lindbergh, began to support this work in the 1930s at the station Goddard established near

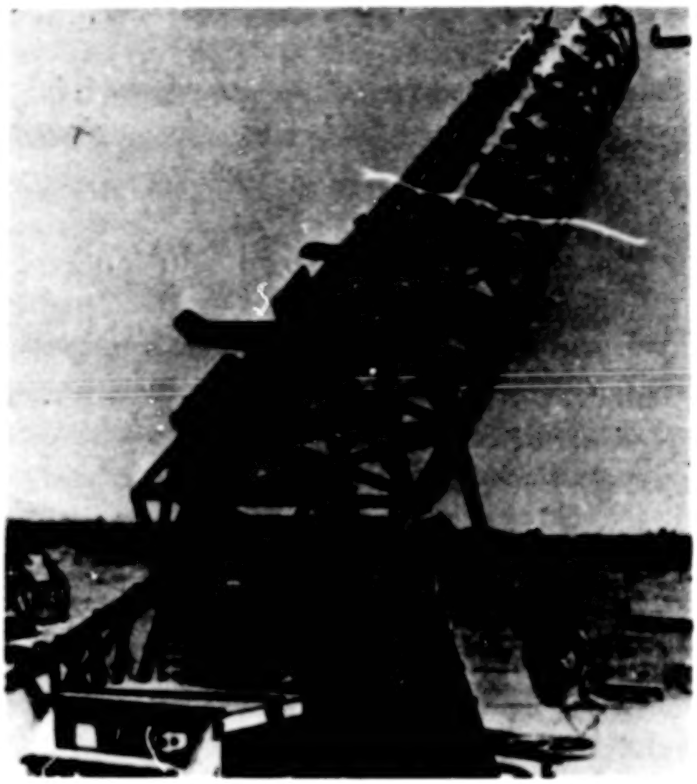


Fig. 8
View of the PRIVATE F in Launcher

Roswell, New Mexico.[34] In 1936, Goddard published a very general report on the progress of his work at Roswell.[35] In 1936 our group at Caltech concluded that Goddard had not succeeded in constructing a successful sounding rocket because he had underestimated the difficulties involved--the day of the isolated inventor of complex devices was over.[1] Even so, success for anyone in the 1920s and 1930s was not likely because of the state of rocket technology--there were no high-thrust, short-duration booster rocket engines, and no suitable guidance systems. Goddard's LOX-gasoline rocket engines, moreover, did not provide a high enough specific impulse for the task.

Similar difficulties confronted experimenters in Germany and in the Soviet Union, where a group headed by M. K. Tikhonravov first launched a liquid-propellant "Rocket 09" in vertical flight on August 17, 1933.[36] In Germany, the V-2 (A-4) ballistic missile was launched in the early 1940s in vertical flight. Though not designed as a sounding rocket, it was so used for a period in the USA beginning in April 1946.[33]

But the situation in 1944 differed radically from the one that faced Goddard ten years earlier. Solid-propellant rocket engines for boosters could be taken from wartime armament rockets and used to circumvent the necessity of a guidance system within a

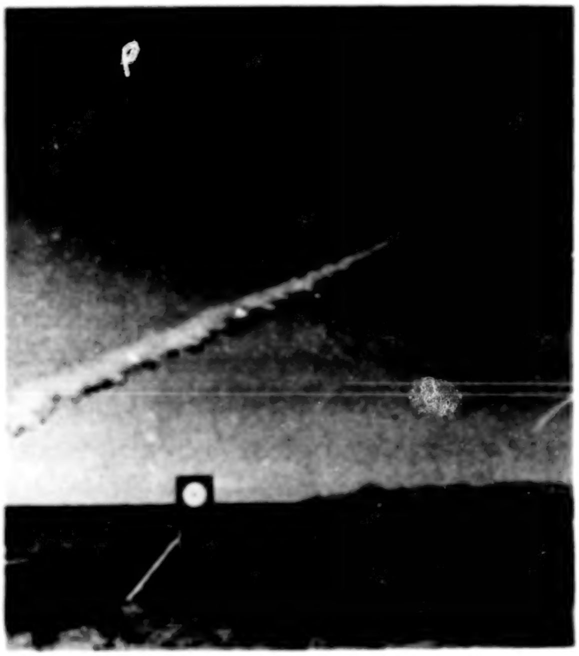


Fig. 9
View of the PRIVATE F in Flight

sounding rocket. Both solid- and storable-liquid-propellant rocket engines with a thrust of sufficient magnitude and duration, and with adequate specific impulse, had been developed at JPL. Then, the Army Ordnance Department requested that JPL study my proposal for a sounding rocket. It was quickly completed and submitted on January 16, 1945 under the title "Considerations of the Feasibility of Developing a 100,000 ft. Altitude Rocket (The WAC Corporal)".[20] The WAC was to be capable of carrying an instrument payload of 25 lb., that would be lowered to the ground by parachute.

We considered the advantages of using various types of rocket engines to propel the WAC, in particular, (a) a high-thrust, short-duration ballistite engine used in armament rockets, (b) a long-duration asphalt-base GALCIT 61-C solid-propellant engine of the type used in the Private A and Private F, and (c) a nitric acid-aniline liquid-propellant engine with a cooled motor and a gas-pressure propellant supply system. Armament rocket engines were ruled out because the use of a single one as the main engine would have produced excessive acceleration of the WAC, which was not desirable from the point of view of any instruments onboard, the mechanical design, and the high drag resulting from high flight speeds in the dense lower levels of the atmosphere. The second alternative, the GALCIT 61-C solid-propellant engine, was eliminated because its weight was excessive. For the WAC to reach an altitude in excess of 100,000 ft., it was estimated that an overall impulse-weight ratio of at least 95 sec. was necessary, and not more than around

77 sec. could be obtained with the available GALCIT 61-C engine. We decided that the most feasible engine to meet WAC objectives would be the nitric acid-aniline engine. The motor to be used was a modified version of one designed by Aerojet to deliver 1,500-lb. thrust for about 45 sec.

The next problem studied was stability of the WAC in vertical flight. Given some small disturbance, gravity would cause the trajectory of a missile in vertical flight to depart more and more from the vertical. Two methods existed to assure vertical flight. The first and most direct method involved using a gyro-stabilization system together with movable aerodynamic surfaces. For the relatively small dimensions of a sounding rocket, however, the weight of such a system with its gyroscopes, servo mechanisms and other auxiliary equipment at the time was so high as to make it a rather dubious method of solving the problem. The second approach involved launching the WAC at a sufficiently high speed so that deviation from the vertical was not of any great importance. This speed could be accomplished by using a second rocket to boost the WAC quickly to a velocity of about 400 ft. per sec. before it left a launching tower. It was estimated that the WAC needed to be guided in a tower for around 60 ft. and that launching acceleration should not exceed about 50 g.

The Ordnance Department accepted these recommendations, and design of the WAC with supporting equipment began immediately. The WAC, as finally constructed and tested (Figures 10 and 11), had the following specifications:[37-40]

Overall length: 194 in.
Maximum diameter of body: 12.2 in.
Three tail fins of 24 in. half span
Gross weight: 665 lb.
Empty weight: 297 lb.
Red fuming nitric acid: 286 lb.
Aniline-furfuryl alcohol mixture: 114 lb.
Air at 1900 psi.: 19lb.
Motor thrust: 1,500 lb.
Thrust duration: 45 sec.
Impulse-weight ratio: 102 sec.

The vehicle was assembled by the Douglas Aircraft Co., Santa Monica, California, from components supplied by JPL's ORDCIT Project.

The WAC sounding rocket was boosted from the launcher by means of a modified ballistite solid propellant rocket engine from an armament projectile called the Tiny Tim (Figure 12), which had the following specifications:[37,40]

Overall length: 96 in.
Maximum diameter of body: 11.75 in.
Three Tail fins of 26 in. half span

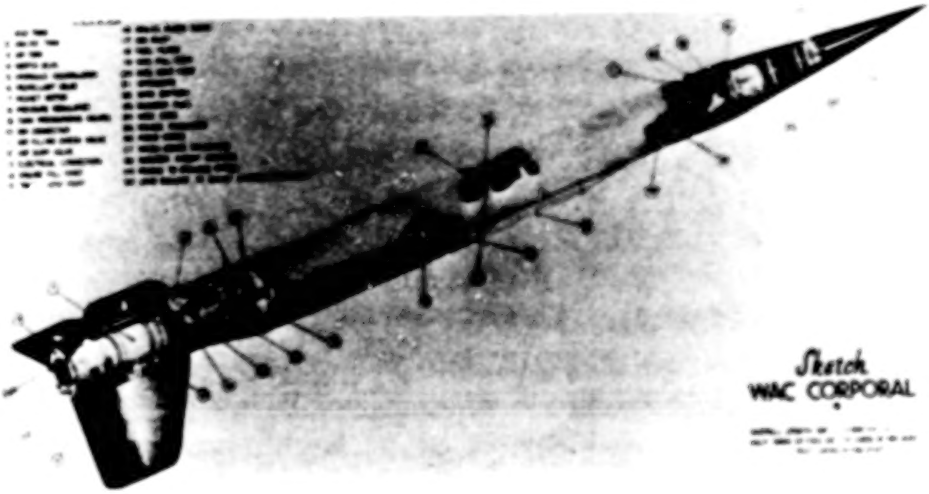


Fig. 10
Sketch of the WAC CORPORAL

Gross weight: 759 lb.

Weight of propellant: 149 lb.

Average thrust (sea level): 50,000 lb.

Nominal duration of thrust: 0.6 sec.

Impulse-weight ratio: 40 sec.

The launcher at White Sands Proving Ground consisted of a 77 ft. triangular structural steel tower, 6 feet on a side, resting on a tripod 25 ft. high with a 26 ft. base giving an overall height of 102 ft.[37,40] (Figure 13). The tower contained three launching rails set 120° apart to guide the WAC and its booster. The effective length of the rails was 82 ft. Details of the design of the launcher are given in Reference 40. A control house was constructed by the Ordnance Department 465 ft. from the launcher (Figure 13).

To check the flight characteristics, tests were made with a 1/5 scale model, called the Baby WAC. It was launched from a scaled-down launching tower at Camp Irwin, California, beginning July 4, 1945.[41] One of the interesting aspects of these tests was the verification of the suitability of employing three instead of the traditional four tail fins. For some reason, aerial bombs and early rockets were equipped with four fins. Stewart, to save weight, proposed we use three fins. An Ordnance "expert" told us the WAC would then be unstable in flight. When Stewart pointed out that arrows for ages had three fins and had performed very nicely, he was still doubtful. The tests of the Baby WAC settled the arguments; the model behaved very well, reaching an altitude of around 3,000 ft.

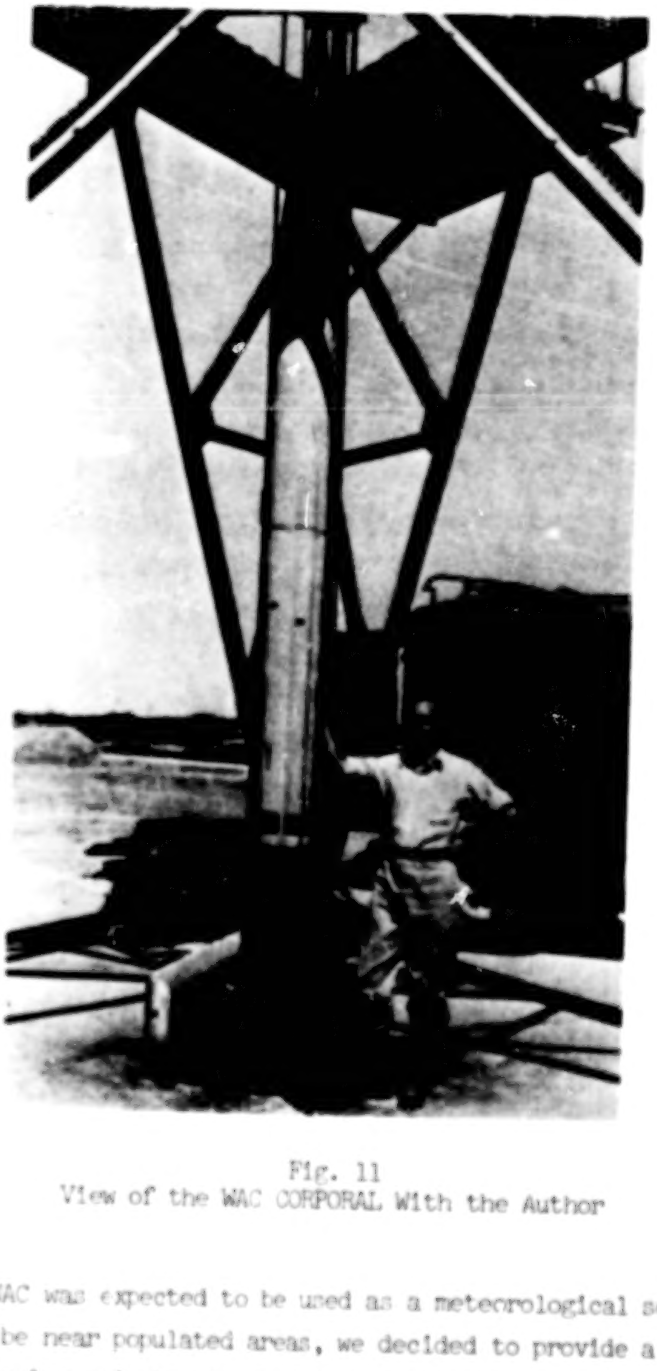


Fig. 11
View of the WAC CORPORAL With the Author

Since the WAC was expected to be used as a meteorological sounding rocket in locations that would be near populated areas, we decided to provide a 10-ft. parachute in the nose of the WAC to lower it to the ground at a velocity of around 70 ft. per sec.[37,42,43] The parachute, to be released at the zenith of WAC's vertical flight, was attached to the top of the propellant tanks and housed in the nose cone. The nose cone was attached to the WAC by means of three explosive pins inserted through the skirt of the nose cone into lugs welded on the tank head. The skirt was seated on a rubber ring

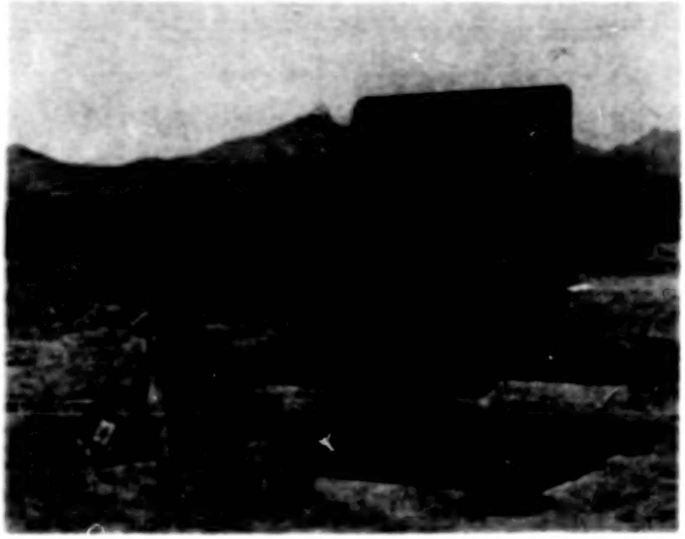


Fig. 12
View of the WAC CORPORAL Booster Rocket

Fig. 13
View of the WAC CORPORAL Launcher, Control House
and Weather Tower, September 1945

seal-strip so that at launching atmospheric pressure was sealed in the nose and provided a force to push the nose away at the zenith of flight, where the outside pressure was practically zero.

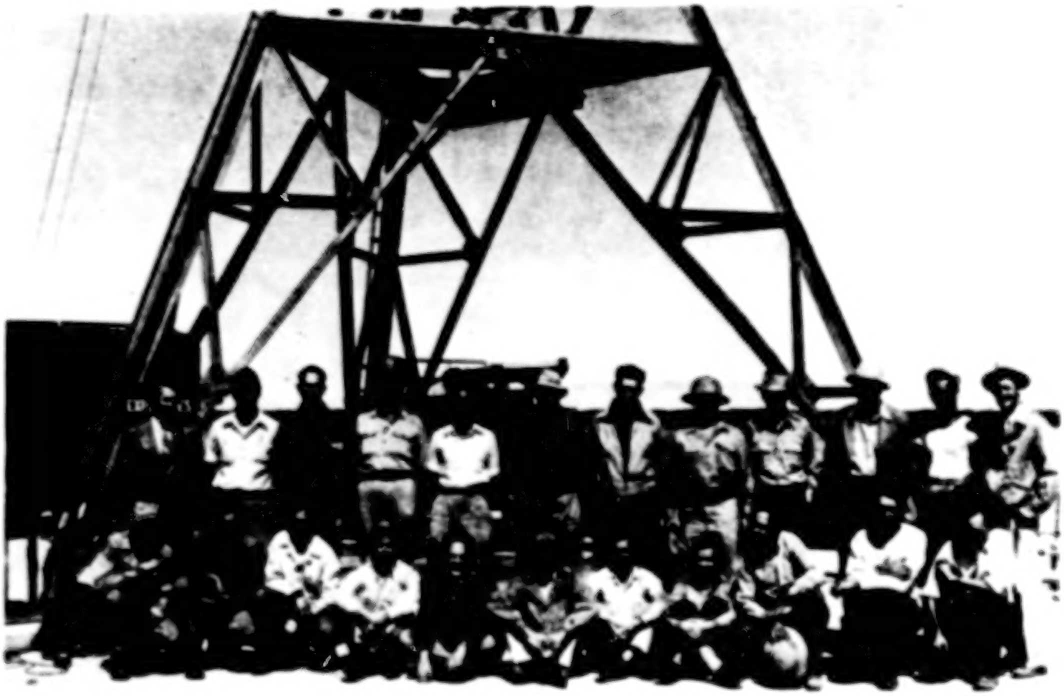
Two schemes were to be tried to fire the explosive pins at zenith. The first used a gyroscope, the frame of which when the WAC turned through 90° was to close an electric circuit to the pins. The second scheme used a mechanical timing device to close the circuit at the predicted time of flight to zenith.[37] The Signal Corps provided radio sonde equipment with its own parachute to be installed in the nose cone, to be released at the same time as the parachute of the WAC.[37] It also provided weather information, including data obtained from balloons rising up to about 100,000 ft.

The WAC was tested at the White Sands Proving Ground in New Mexico during September 26 to October 25, 1945, just nine months after I proposed the sounding rocket to Trichel in Washington, D.C. Details of the test program can be found in References 37 to 44. The Ordnance Department provided a C-47 Douglas aircraft to transport personnel and equipment between Los Angeles and White Sands—it was, of course, called the ORDCIT Airline. A detour was made on one flight so we could see the blast area near Alamogordo, New Mexico, caused by the explosion of the first atom bomb. It was a very disturbing sight, especially for us who were involved in the development of long-range rocket missiles.

In 1936, I had placed on my office wall at Caltech a chart showing the component parts required for a successful sounding rocket. The dream had now become a reality, with an engineer in charge of each of those components. Key members of my WAC team were: M. M. Mills (Booster), P.J. Meeks (Sounding Rocket), W.A. Sandberg and W.B. Barry (Launcher and WAC Nose), S. J. Goldberg (WAC Field Tests), H. J. Stewart (External Ballistics) and G. Emmerson (Photography).

A group photograph of most of the ORDCIT Project personnel who participated in the test program at White Sands is shown in Figure 14, and the organization chart of the groups participating in the WAC Corporal program in Figure 15. The large number of people involved in this program indicates why the dreams of individuals and small groups of rocket enthusiasts in the 1920s and 1930s to design, construct, and test a high-altitude sounding rocket had little chance of success. Fortunately, most pioneers do not foresee all of the practical implications of their dreams.

The test program proceeded step by step until we were confident that basic components were satisfactory. First, four rounds of the weight-adjusted Tiny Tim booster alone were fired to check the booster, launcher and firing controls, and to give practice to radar and camera crews. Next, two dummy rounds of the WAC constructed of steel tubing filled with concrete were launched. These were followed by two WAC rounds with a partial charge of propellant to check behavior of the liquid-propellant rocket engine, the separation of the booster from the WAC and the operation of the nose cone release mechanism. Up to this point, all systems operated satisfactorily except for the nose cone release mechanism, which failed both times. The partial charge WAC reached an altitude of about 28,000 ft., though difficulties were encountered in tracking it with radar.[37,44] Radar tracking



[illegible]

[illegible]

[illegible] of the WAC [illegible] [illegible] of radar tracking data, it was esti- [illegible] ft. [illegible] of the WAC [illegible] was about [illegible] ft. [illegible] vertical [illegible]

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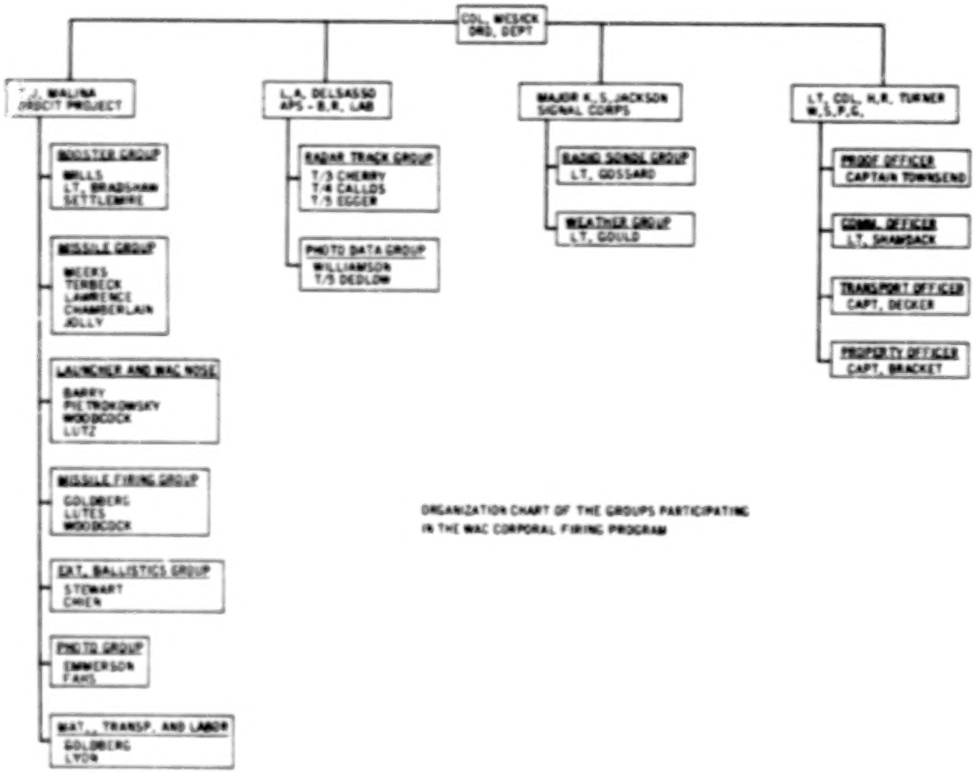
COL. WESICK
ORD. DEPT
F.J. MALINA
ORDCIT PROJECT
BOOSTER GROUP
WELLS
LT. BRADSHAW
SETTLEMIRE
MISSILE GROUP
WEEKS
TERBECK
LAWRENCE
CHAMBERLAIN
JOLLY
LAUNCHER AND WAC NOSE
BARRY
PIETROKOWSKY
WOODCOCK
LUTZ
MISSILE FIRING GROUP
GOLDBERG
LUTES
WOODCOCK
EXT. BALLISTICS GROUP
STEWART
CHIEN
PHOTO GROUP
EMMERSON
FAHS
MAT., TRANSP. AND LABOR
GOLDBERG
LYON
L.A. DELSASSO
APS - B.R. LAB
RADAR TRACK GROUP
T/3 CHERRY
T/4 CALLOS
T/5 EGGER
PHOTO DATA GROUP
WILLIAMSON
T/5 DEDLOW
MAJOR K.S. JACKSON
SIGNAL CORPS
RADIO SONDE GROUP
LT. GOSSARD
WEATHER GROUP
LT. GOULD
LT. COL. H.R. TURNER
W.S.P.G.
PROOF OFFICER
CAPTAIN TOWNSEND
COMM. OFFICER
LT. SHAWSACK
TRANSPORT OFFICER
CAPT. DECKER
PROPERTY OFFICER
CAPT. BRACKET
ORGANIZATION CHART OF THE GROUPS PARTICIPATING
IN THE WAC CORPORAL FIRING PROGRAM

Fig. 15
Organization Chart of the Groups Participating in the WAC CORPORAL
Test Program at White Sands Proving Ground, New Mexico
Between 26 September and 25 October 1945

astronautical fraternity, the tradition of lighting up a cigar had not as yet begun. We contented ourselves by letting out whoops of joy, shaking hands and patting each other on the back. Our celebration was of short duration. We quickly returned to work to prepare for the launching of the next round of the WAC the next day!

Six charged rounds were fired during this 1945 program. In round 7, the nose cone release mechanism functioned prematurely at about 90,000 ft. and in round 8 the nose cone was released prematurely shortly after the WAC left the launcher but it continued in vertical flight. The parachute did not lower the WAC to the ground successfully in any of these flights, for it either failed to release, released prematurely, or tore off during descent. This did not surprise us, as there was very little experience on the behavior of parachutes opened at high altitudes. When the parachute failed the first time, instead of seeking cover, many of us remained standing in the impact area because Homer Stewart assured us that the probability of being struck was extremely small. Even so, we were startled to see one impact take place about 200 yards from where we were standing.

When the center of gravity of the WAC was too far forward it went into a flat spin during its fall to the ground and, as a result, it struck the ground at greatly reduced speed. Round 6, in remarkably good condition after impact, is shown in Figure 16.

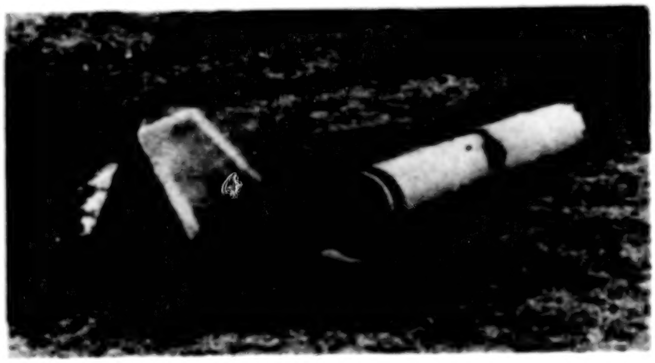

Fig. 16
View of Round 6 of the WAC CORPORAL After Impact on the Ground

The last flight, round 10, was made at night. A view of the jet trail above the launcher is shown in Figure 17. The plot of radar data obtained for this flight is shown in Figure 18. Radar tracking gave a minimum of data to permit the altitude to be calculated. On most rounds it failed completely. The fact that radar now can be used to map the surface of a distant planet seems incredible.

The test of the WAC Corporal showed that design features we chose for a practical sounding rocket of reasonable cost were correct. The use of a booster rocket and an adequate launching tower circumvented the need of a guidance system. The use of a storable liquid propellant engine simplified launching procedures compared to the use of LOX engines.

It is a tradition at JPL to be conservative and unsensational in making proposals when dealing with problems involving many unknowns. Thus, we set the altitude to be attained at above 100,000 ft. Since no liquid-propellant engine sounding rocket before had reached more than a few thousand feet, that seemed quite ambitious. The fact that the WAC doubled our first theoretical altitude estimates was primarily due to a reduction of the empty weight and the increased amount of propellant carried.

The recommendations made as a result of the tests included:

1. Shifting the compressed air tank from the bottom to the top of the tank assembly in order to shift the center of gravity forward.
2. Suggestions for further reducing the empty weight of the vehicle.
3. Designing of a reliable nose cone release mechanism and of a stronger attachment of the parachute to the vehicle.

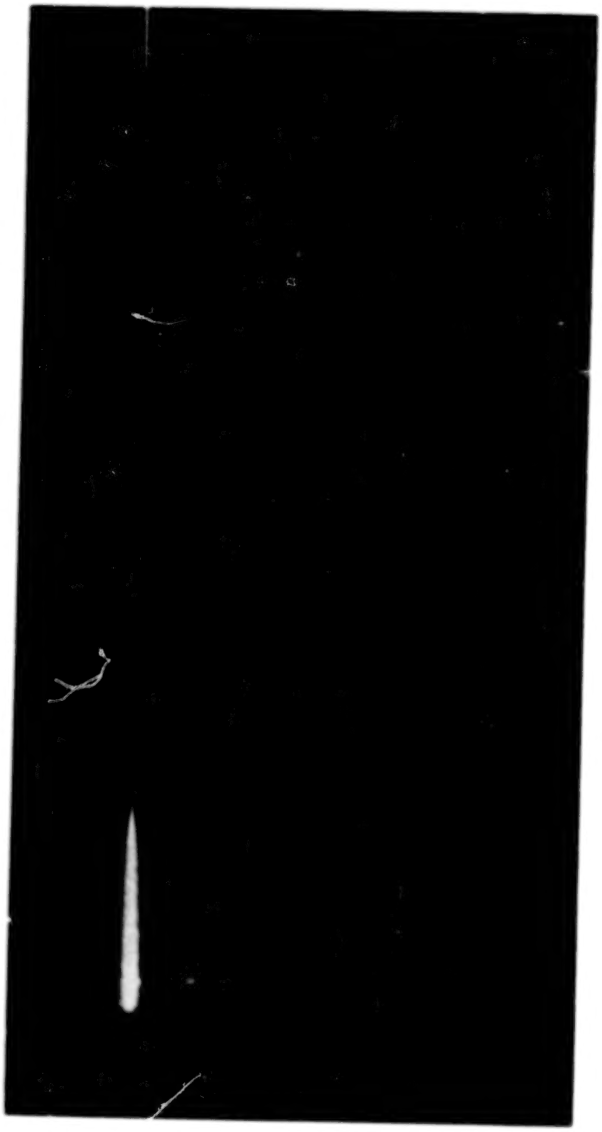

Fig. 17
View of the Night Flight of the WAC CORPORAL on 25 October 1945

4. Provision of a radar beacon on the vehicle to facilitate radar tracking.
5. Design of a booster rocket with an improved impulse-to-weight ratio.

When tests of the WAC concluded in October, we began studies for another sounding rocket given the designation Sergeant. It was to be about the same size as the WAC but with a new cooled or uncooled motor, perhaps a ducted rocket motor, and a gas-generation-propellant supply system. This program was not pursued, and Sergeant later became the name of a solid-propellant guided missile designed at JPL.

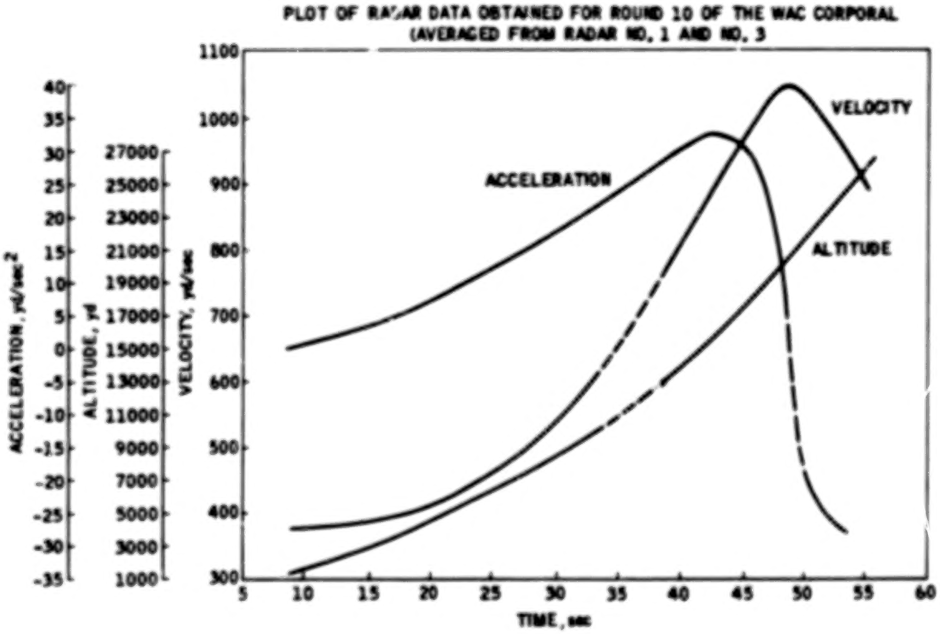

Fig. 18
Plot of Radar Data Obtained From Round 10 or the
WAC CORPORAL Fully Charted With Propellant

On November 9, 1945, at Aberdeen, Maryland, representatives of the Ordnance Department and the Signal Corps met with JPL personnel to review the results of the WAC tests.[46] It was decided that another five rounds of the WAC should be assembled and used for testing an improved parachute and parachute release system, a remitter beacon in the nose for radar tracking, and a data telemetering system. Various tests were conducted in connection with this program at White Sands Proving Ground between May 7-29, 1946.[45,46]

While these tests were underway, we first met members of the German V-2 development team who had been brought to the U.S.A. to be incorporated into the Ordnance Department's long-range missile program. Wernher von Braun acted as spokesman for the group. By this time we were quite well acquainted with the V-2 missile. Two complete ones were received at JPL in June 1945. Their arrival on two railroad flat cars caused more excitement among the people of Pasadena than a whale similarly transported some time before.

In March 1946, plans were initiated for the design and construction of the unproved WAC Corporal B sounding rocket, with Meeks as Project Coordinator. The vehicle incorporated the recommendations resulting from the tests of October 1945, and later, those of May 1946, as well as a lighter weight redesigned engine and propellant tanks to increase the propellant-to-gross weight of the first design. A report on the tests of the WAC B at White Sands Proving Ground in December 1946 and in February - March 1947 can be found in Reference 47.

The results and know-how obtained with the WAC Corporal were incorporated in its successor, the Aerobee, designed and constructed by Aerojet and assembled by the Douglas Aircraft Co. That project, sponsored by the Navy Bureau of Ordnance, was carried out under the technical direction of the Applied Physics Laboratory of Johns Hopkins University, supervised by James A. Van Allen. The contract to Aerojet was awarded on May 17, 1946, and the first full-scale Aerobee was launched at White Sands on November 24, 1947.[33,47] Since 1948, numerous variations of the basic WAC design have been constructed in the USA and in other countries for use in high-altitude research.[33]

IX. THE CORPORALS

A major objective of the ORDCIT Project, as described in Section III, was the development of a remotely controlled missile to carry an explosive load of 1000 lb. for a distance of up to 150 miles, with a dispersion not in excess of 2 percent and at a velocity sufficient to afford protection from fighter aircraft. On August 14, 1944, Tsien outlined a program for an experimental missile (with the designation XF36 L 20,000) that had the following tentative specifications:[48]

Gross weight: 5 tons
Diameter: 36 in.
Rocket thrust: 20,000 lb.
Thrust duration: 60 sec.
Sp. propellant cons: 0.005 sec.
Stabilization: fins
Range: 30 to 40 miles

At this time, only a storable-liquid-propellant engine of the type developed by JPL could meet these specifications; the thrust required was much higher than any motor constructed in the U.S.A. up to that time. The largest uncooled motor that had been tested at JPL delivered about 5,000 lb. thrust.

We also faced a requirement to launch a large rocket vertically without a booster rocket and a guiding launcher. I do not believe that we knew in early 1944 that the V-2 was launched in this way. There was considerable scepticism voiced over the possibility of keeping a large missile in a vertical position solely by means of tail fins and control surfaces as it slowly lifted off the ground. On the other hand, it was not feasible to boost a large, lightly constructed vehicle at a high velocity.

Detailed analysis of the various components and of the flight characteristics of the missile began immediately. The design and testing of the 20,000-lb.-thrust nitric acid-aniline type engine was initiated under the supervision of Howard Seifert. We decided to develop two engines simultaneously: an engine with a gas-pressure propellant supply system, with which much experience had already been gained, for installation in a missile designated the Corporal E, and an engine with a turbine-driven-pump propellant supply system engine (turborocket) for installation in the Corporal F. The development of a turborocket engine, already underway for some time by N. Van de Berg, was speeded up.[2] Facilities for testing the complete Corporal engines were constructed at the Muroc Flight Test Base, California, of the Air Technical Service Command. They were completed in June 1945 and operated under the direction of W. B. Powell.[12]

When Dunn became Assistant Director in 1944, he devoted much of his effort to the Corporal program. In the summer of 1945, Summerfield returned from Aerojet and became coordinator of the program.[45] Information on work carried out by the end of 1946 on the aerodynamics and mechanical design of the Corporals can be found in the archives of JPL.[48-64] Fabrication of components of the missiles was sub-contracted to machine shops in the Southern California area. The construction of components became the main bottleneck in the program because the ORDCIT Project at this time could not compete with priorities assigned to other production orders of the Armed Forces for the final year of war in the Pacific Theater of Operations.[12]

The problems of remotely guiding and controlling a missile were entirely new to JPL and, furthermore, no work had been carried out on aircraft autopilot systems at the Caltech Guggenheim Aeronautical Laboratory. Upon the suggestion of Skinner, consideration was given to making contractual arrangements on Corporal guidance development with either C. Stark Draper's group at the Massachusetts Institute of Technology (M.I.T.) or the Sperry Gyroscope Co.[12] + Meetings between von Kármán and Trichel on July 29, 1944, and Martel and the author on August 24 with Sperry personnel, led to a contract between JPL and Sperry for the cooperative development of the Corporal guidance system.[12] I especially enjoyed making the acquaintance of Gifford E. White of Sperry, who, with Pickering, laid the basis for the guidance system.[66] C. B. Millikan, who had wide experience in aerodynamics, devoted much time to getting this program underway. Primarily through his efforts, Pickering joined the staff of JPL in August 1944 to establish the Remote Control Section, with Frank Lehan as his principal assistant. Information on the work carried out under this Section up to the end of 1946 can be found in the archives of JP.[45,66]

I believe memoirs by Dunn, Pickering, and Summerfield on the Corporal programs would be most valuable, for they were responsible for its development after 1946.++ The

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+See C. Stark Draper, "The Evolution of Aerospace Guidance Technology, 1935-1951: A Memoir," in this volume - Ed.

++See William H. Pickering with James H. Wilson, "Countdown to Space Exploration: A Memoir of the Jet Propulsion Laboratory, 1944-1958," in this volume - Ed.

Corporal was successfully fired at White Sands on May 22, 1947 (cf. Figure 19), becoming a tactical weapon of the U. S. Army in April 1954.

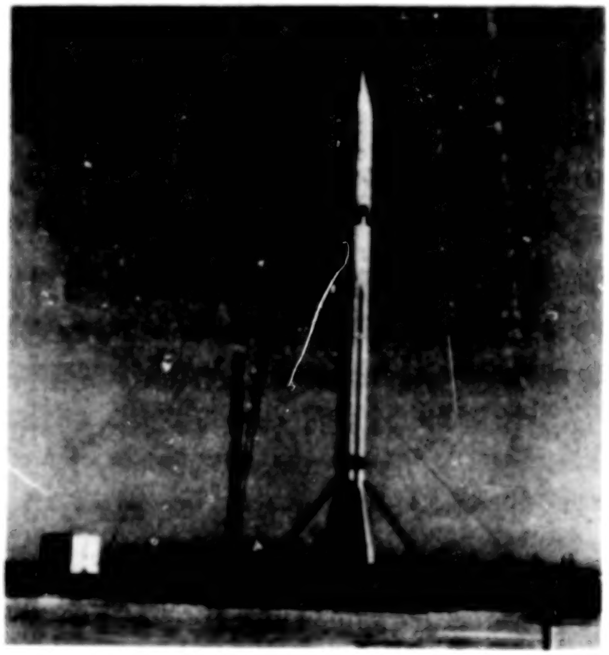

Fig. 19
The CORPORAL Guided Missile

X. SOLID-PROPELLANT RESEARCH AND APPLICATIONS

Sponsorship of solid-propellant research was taken over by the ORDCIT Project from the Air Force Materiel Command on July 1, 1944.[2] By this time, JPL had made the following fundamental contributions to the design and construction of long-duration solid-propellant engines:[2]

(a) Theory
  1. Von Kármán-Malina theory of constant-thrust long-duration engines (1940)

(b) Propellant development
  1. Parsons' break-away from ballistite with amine black powder (1940)
  2. Parsons' introduction of perchlorates as an oxidizer (1942).
  3. Parsons' introduction of asphalt as a fuel-binder with perchlorates; the invention of a castable case-bonded composite propellant charge (1942).

(c) Engine component design
  1. Parsons' design of a restricted-burning (case-bounded) propellant charge with amine black powder (1940).

2. Malina-Mills design of a safety pressure relief valve (1942).
3. Mills' review of various types of burning surfaces of a charge and theoretical confirmation that the surface of a cigarette-type burning charge was stable (1943).

After the successful JATO development with the asphalt-perchlorate propellant in 1942, Mills sought a fuel-binder for the perchlorate superior to asphalt. In 1944, Charles Bartley joined Mills' group, and in 1945 introduced as a replacement for asphalt a castable elastomeric material, polysulfide rubber, produced by the Thiokol Chemical Corporation. A report on the development of this propellant can be found in Reference 67. The polysulfide rubber compared to asphalt produced a propellant much better both as regards storage temperature limits and hardness at high atmospheric temperatures. The latter property was especially important in the design of high-thrust engines requiring a charge with an internal-burning surface rather than a cigarette-burning surface.[68] Since at this time only Aerojet in the U.S.A. was producing composite solid-propellant engines, I drew the company's attention to the asphalt replacement, but it was already interested in a similar material made by the General Tire and Rubber Co. I believe it was at the urging of the Ordnance Department that the Thiokol Chemical Corp. entered the field of composite solid propellants with the new fuel-binder found at JPL.

After obtaining the experience with the composite solid-propellant missiles Privates A and F, studies began at JPL in 1946 on larger missiles using, in particular, the polysulfide rubber-perchlorate type of propellant. The results of these studies[69] led eventually to the design of the tactical guided missile, Sergeant.

The Laboratory followed closely developments with other types of solid propellants, especially ballistite, used in high-thrust short-duration engines suitable for boosters. Available engines were modified to meet special requirements for boosting the Privates and the WAC Corporals.

Considerable research was also conducted by the Solid Propellant Rocket Section under Bartley, and the Propellant Section under N. Kaplan, and later under S. A. Johnston, on gas generation systems to replace stored gas for feeding liquid propellants to rocket motors. Our optimism that such a system could be developed quickly proved to be unfounded (See Section VIII).

XI. APPLICATION OF ROCKET PROPULSION FOR EXTRATERRESTRIAL SPACE EXPLORATION

I have described the background of the initiation of rocket research at Caltech in my first memoir on the GALCIT Rocket Research Project, 1936-38.[+] Space travel, which was the goal of this Project, was not stressed after we realized that existing rocket technology was insufficient to reach this goal. It is true that journalists published stories interpreting our studies of sounding rocket performance and preliminary rocket engine experiments as heralding a planned landing on the Moon by Caltech. It was not until after work on the Corporal was initiated in early 1945 that studies resumed that had been put aside in 1938 (See Section V).

The WAC could be considered as a two-step rocket vehicle. Summerfield and I began, I believe, in the summer of 1945 a more detailed analysis of such vehicles, with a re-evaluation of the feasibility of a rocket payload being launched at suffucient velocity to escape the gravitational field of the Earth. Our analysis was based on the state of rocket technology at that time, and included a discussion of the possible use of a nuclear energy rocket engine.[70] The analysis led to the Malina-Summerfield Criterion for step-rockets, which states that the optimum step-rocket will be one in which the ratio of the mass of payload for each step to the mass of the step propelling the payload is the same (the payload for step one is the mass of all succeeding propulsion steps plus the mass of the final useful payload).

We calculated, as an example, a step-rocket to launch to escape velocity a useful payload consisting of an instrument for measuring cosmic ray intensity with a radio beacon transmitter for sending the data back to Earth. Obviously, if a useful payload could be launched to escape velocity, it could also be placed in orbit around the Earth. JPL's first satellite, Explorer 1, on January 31, 1958, carried a cosmic ray instrument, and the payload weight was about the same as we had chosen. On January 3, 1946, in Washington, D.C., I presented the results of our study, as well as the high points of achievements at JPL, to the War Equipment Board of the Army, headed by General Joseph W. Stilwell. As I recall, the Board made little comment on the implications of the possibility of launching a man-made object away from the Earth. Stilwell observed in his diary in regard to his assignment to the Board: "I am eminently suited to do something else and would as lief sit on a tack."[71]

[+] Frank J. Malina, "On the GALCIT Rocket Research Project, 1936-1938," First Steps Toward Space: Proceedings of the First and Second History Symposia of the International Academy of Astronautics, Smithsonian Annals of Flight, No. 10, Washington, D.C., 1974; also in Russian in From the History of Rockets and Astronautics (Moscow: Publishing House Nauka, 1970).

We had made conservative assumptions in our "escape" analysis, especially as regards propellant specific impulse and structural weights of vehicle components. We estimated that a 5-step rocket to launch a 10-lb. payload to escape velocity would have to weigh 3,000,000 lbs. for the nitric acid-aniline propellant combination, and that 450,000 lbs. would be required for oxygen and ethanol. It was difficult for almost anyone in 1946 to imagine meeting the engineering problems and cost of constructing such rockets.

The analysis we made was correct, as J. E. Froehlich pointed out in 1959.[72] However, between 1946 and 1959 improvements in engines and structural design permitted the gross weight required to launch a payload to escape velocity to be reduced by a factor of over 400. This is certainly an amazing demonstration of the possibilities of technological research and development when there is a will to support them--for good or evil purposes.

On July 7, 1946, I returned to London on a second mission in Europe for the Army Ordnance Department. Assigned to the office of Colonel Reed, Assistant Military Attache, I was asked to report on matters related to science and technology as well as rocket propulsion and missile design. I remained in Europe until December 1946. During this time I visited von Kármán several times and we discussed aspects of the post-war situation, especially as they affected our plans for the future.

I discussed our "escape" study at a meeting of the British Interplanetary Society (BIS) in London, and presented the paper formally at the Sixth International Congress for Applied Mechanics in Paris.[70] Members of the BIS who attended the meeting were not very happy when I said that at this time one could land a man on the Moon, provided he was sent up in two halves in separate rockets, without the offer of a return trip to Earth.

One result of this study was Summerfield's suggestion that a program be initiated to launch a two-step rocket vehicle consisting of the WAC Corporal boosted by the V-2. Initiation of this program at JPL was authorized by the Ordnance Department in October 1946, and the vehicle was designated Bumper WAC. A photograph of the rocket is shown in Figure 20. It was successfully launched at White Sands Proving Ground on February 24, 1949 and the WAC reached an altitude of 244 miles.[73] Thus the WAC became the first recorded man-made object to enter extra-terrestrial space, and the "space age" could be said to have been opened in the U.S.A. in 1949. On July 24, 1950, a Bumper WAC became the first missile launched from Cape Canaveral.

While Summerfield and I were concluding our "escape" study, the Navy Bureau of Aeronautics on 12 December 1945 made a contract with JPL for studies of a rocket vehicle for launching an Earth satellite. The work of the Navy and of JPL on this program can be

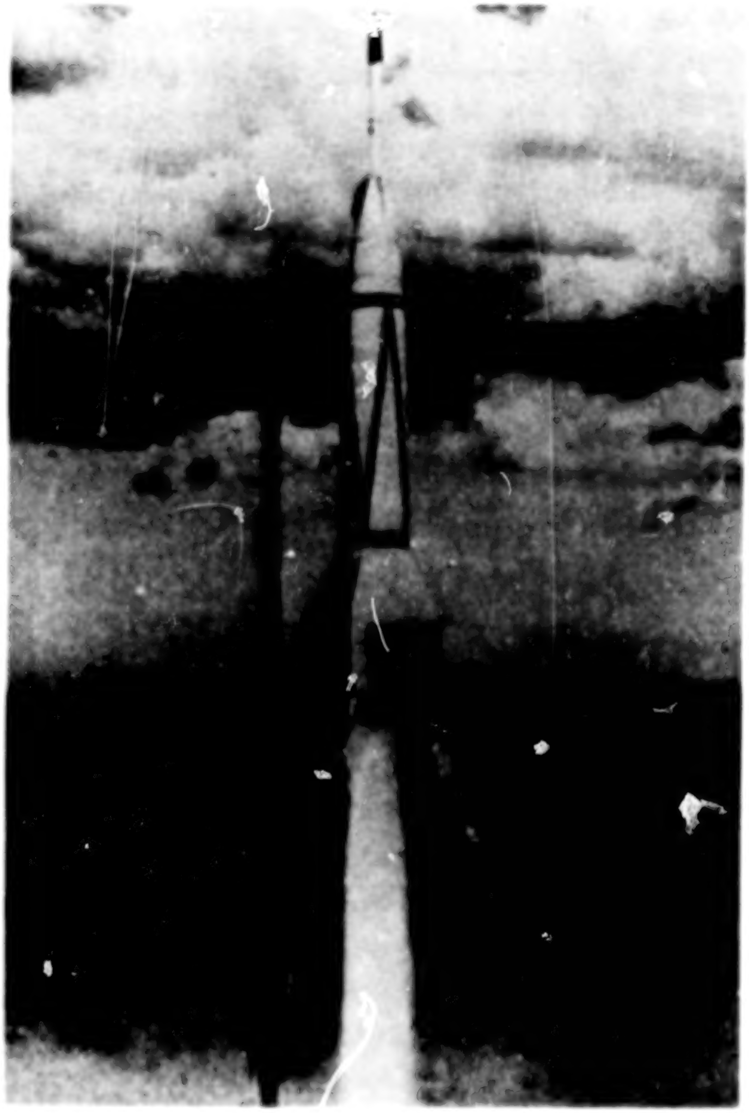

Fig. 20
The BUMPER-WAC Rocket Vehicle

found in a detailed historical paper by R. Cargill Hall in this volume. Unfortunately, for the reasons he puts forward, the Navy dropped the program in 1947.

A summary of the basic aspects of the physics of space flight as understood at JPL in 1946 can be found in a paper by Seifert, Mills and Summerfield.[75] A Guided Missile and Upper Atmosphere Symposium was held at JPL between March 13-16, 1946, in which experts from all parts of the USA participated.[76]

XII. CONCLUDING REMARKS

There are many areas of research conducted under the ORDCIT Project that I have not discussed in detail, for example, work on materials, chemistry of propellants, ramjet design, telemetering of data from missiles in flight and remote control of missiles. The main reason is that my role in these domains was mainly of an administrative character and, therefore, those that led the actual work would best be able to shed light on this work.

The preparation of my three memoirs on the origins and work the JPL between 1936-46 has heightened my appreciation of the difficulties confronting historians of science and technology. Not only must historians understand the technical matters of a development, but they must make interpretations requiring wide historical perspective. If, in addition, these historians wish to portray the events accurately for the lay public as well as technical scholars, then their task is a most difficult one indeed--and if great care is not exercised, the truth will be replaced by myth.

APPENDIX

CONTENTS OF CALTECH'S PROPOSAL OF 28 FEBRUARY 1944
TO THE ORDNANCE DEPARTMENT (cf. Reference 12)

1. Theoretical investigations on the possible range as a function of the initial weight and the ratio between warhead and initial weight. It is especially necessary to decide whether pure projectiles or wing missiles or both types should be developed.

2. Theoretical and experimental investigations on stability and aerodynamic control. Study of tail stabilizers for projectiles and wing missiles; spinning devices for pure projectiles.

3. Development of an adequate propulsion system. Liquid rockets for indefinite duration, and solid rockets up to 45-second duration have been developed by the GALCIT Project and the Aerojet Engineering Corporation....

4. Study of the launching system; development of adequate guiding rails and auxiliary launching equipment.

5. Construction of model projectiles of moderate size (300 to 2,000 pounds) in order to obtain data on drag, stability, propulsion efficiency and dispersion for the prototype project.

6. Methods of remote control.

7. Development of adequate experimental technique for firing tests, execution of firing tests, and evaluation of results.

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It is believed that the Institute could undertake the pursuit of these objectives. As far as items 1 to 5 are concerned, the Institute would undertake, with its own adequately enlarged personnel, theoretical and experimental development work and would subcontract in the Los Angeles area the design and construction of launching equipment and actual models destined for firing tests. A preliminary survey of available shop and manpower facilities in the local industry indicates that such a procedure would be feasible. As to items 6 and 7 it will be necessary that the Ordnance Department select and maintain sites for the firing tests, furnish personnel and materials for execution of the tests, and especially instrumentation and personnel from the Ballistic Research Laboratory for the ballistic measurements.

The laboratory development work should be carried out on a tract owned by the Institute adjoining the Air Corps Jet Propulsion Research Project. Ample space is available for the necessary facilities for laboratory work and ground tests on propulsion systems.

As a tentative proposal, the following items are respectfully submitted:

A. The Institute's Responsibilities

I. To furnish comprehensive reports on the following items:

   a. On the possible range and bombing load of large-size rockets.

   b. On stability and aerodynamics control of such rockets.

   c. On characteristics of adequate propulsion systems.

   d. On the characteristics of various launching systems.

II. To design and construct the necessary facilities and carry out experimental research on propellants and materials involved in the design of long-duration rockets and athodyds; to carry out ground tests on the characteristics of such devices; to carry out and/or direct and supervise wind tunnel and airplane flight tests on athodyds.

III. To establish basic engineering data for the launching devices selected for firing tests.

IV. To establish basic engineering data for the model projectiles to be used in firing tests.

V. To supervise the design and construction of model projectiles and launching equipment subcontracted to engineering and manufacturing organizations selected by the Institute.

VI. To set up a program for the firing tests and cooperate in carrying out the tests and evaluate the results based upon all proof data obtained.

VII. a. To establish the specifications and basic engineering design data for for one or more prototype units.

   b. To supervise the design and construction of prototype units and pertinent launching equipment by engineering and manufacturing organizations selected by the Institute.

B. The Ordnance Department's Responsibilities

I. To furnish the necessary funds for the facilities, materials, supplies, salaries, wages, and other expenses including a fee covering the overhead involved by the general administration of the Institute.

II. To select, equip, and maintain suitable sites for firing tests.

III. To furnish personnel, materials, and supplies for execution of the firing tests including the instrumentation and personnel from the Ballistic Research Laboratory for the necessary ballistic measurements and to be responsible for all safety precautions.

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COUNTDOWN TO SPACE EXPLORATION:
A MEMOIR OF THE JET PROPULSION LABORATORY, 1944-1958[+]

William H. Pickering[++] with James H. Wilson (USA)

I. INTRODUCTION

In the spring of 1944 I accepted the invitation of Theodore von Kármán, Director of Caltech's Guggenheim Aeronautical Laboratory (GALCIT), to join a new wartime research and development project. It had been stimulated by the Allies' discovery of a large and vigorous rocket-missile effort which the Germans soon launched upon the world under the name of V-2. Although Kármán and his associates in the GALCIT Project No. 1 had developed a family of small rocket engines for aircraft use, another Caltech project had devised a series of short-range rocket projectiles, and other U.S. technical groups had done important work in military rocketry, at the time no development or research in the field of long-range rocket vehicles and missiles existed in this country to match the challenge of the V-2. Kármán proposed that such a program be started, and Army Ordnance offered to sponsor the research at what was soon named Caltech's Jet Propulsion Laboratory.[1]

Many of the skills to be brought to bear on the long-range rocket projectile were already a part of the GALCIT Project, which had made major contributions to the technology of rocket propulsion, notably in castable restricted-burning solid rockets and storable liquid-propellant engines.[2] But the new objective called for new technical skills. The brilliant applied mathematician Hsue-Shen Tsien, who had contributed with Kármán and Frank Malina to the JPL-Ordnance proposal, organized a Research Analysis Section; problems of air-breathing engines, aerodynamic testing, and structural design, would be considered in other new sections; field testing and the acquisition of a test range were new problems. My own field, electronics and instrumentation, would also be heavily engaged in the Remote Control Section that I was to found.

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At the time I spoke with von Kármán and agreed to join the new project, I was a member of Caltech's Electrical Engineering faculty engaged in a variety of teaching and research activities, mostly war-related. I had training programs underway for military electronics officers and civilian technicians; I had been associated in radar development, both at Caltech and at the MIT Radiation Laboratory; and I had worked actively in the 1930s and early 1940s with Victor Neher and Robert Millikan in cosmic-ray surveys using balloon-borne sensors. These interests fitted in rather well with the anticipated needs of the Jet Propulsion Laboratory, and it was very pleasing to be able to bring them together in this fashion. I am not certain whether I recognized the high-altitude rocket and its progeny the spacecraft as practical successors to the cosmic-ray balloon, but I feel certain that Kármán did see this far ahead.

Theodore von Kármán was many great men: a scientist and engineer, a teacher whose students may still be found among the leaders of aeronautics in many nations, a gracious gentleman and persuasive advocate, and a builder of institutions such as the International Academy of Astronautics.[3] But I think his greatest skill was as a mixer of intellectual disciplines and social forces. His personality drew me into the field of rocket technology, and created the Jet Propulsion Laboratory as a permanent link between ordnance and rocketry users and the scientists of Caltech.

This two-dimensional mixing process, between rocket technology and the applied sciences on the one hand and between Caltech and the government on the other, offers both a perspective for viewing the growth of the Laboratory from the challenge of the V-2 to the launch of Explorer 1, and a key for understanding the results. It is my purpose in this memoir to review the stages of this growth and to reflect on the mixing process as part of the preparation for the exploration of space.

II. A FOCUS FOR RESEARCH

The project to study the technical problems of a long-range rocket projectile, named ORDCIT after its Ordnance sponsor and Caltech as the research institution, came to life under contract in late June 1944. At that point it shared the research and test facilities with three other projects sponsored by the Army Air Forces. These were the continuing investigation of rocket propulsion (the original GALCIT Project No. 1), the study of an underwater rocket projectile under Louis Dunn's leadership, and research into the air-breathing ramjet engine. Cooperation among the sponsoring agencies did much to promote the joint and mixed research activities.

In part because of the broad spectrum of technical questions it raised and the new research it called for, and partly because of the strong appeal, both scientific and strategic, of ultimately being able to develop a high-altitude, long-range rocket vehicle, ORDCIT enjoyed an advantage over the three propulsion projects. A high-altitude sounding rocket would carry on the work of Malina and his pre-war colleagues, while the long-range missile answered the direct challenge of the V-2. Although all four of the projects continued to grow, this last one grew fastest and furthest. It grew geographically, expanding to temporary flight-test ranges in the Mojave desert and the Texas reaches of Fort Bliss, and to permanent launch facilities at White Sands, New Mexico; it grew technically, over the range of engineering disciplines mentioned above; and operationally, from the testing of research vehicles to the systematic integration of a new kind of weapon, and to new dimensions of scientific observation.[4]

My own efforts were concentrated upon the ORDCIT Project from the time I joined the Jet Propulsion Laboratory late that summer. My first task was to travel East to examine radar and optical tracking techniques at the MIT Radiation Laboratory and Aberdeen Proving Ground, look into the state of remote control equipment at Sperry Gyroscope and Gulf Industries, and see Theodore von Kármán, who was recuperating from surgery in New York. I concluded that we would develop the necessary ground-test instrumentation and flight-data information systems locally at JPL.

It is important to recall that the focus of the pre-war electronics industry was upon commercial broadcast and communications technology; television and feedback-controlled automation were on the bench, not on the shelf. High-frequency applications—such as radar was to be—were severely limited by the lack of an appropriate amplifying device. It was impossible to buy, and difficult to develop, equipment that would function reliably under the stresses of field operation or rocket flight. But wartime mobilization changed this completely.

During this period the state of electronics technology advanced rapidly, almost violently. Anglo-American collaboration made possible a large and growing family of radar equipment and widened fields of application. Components rugged enough to ride an artillery shell, exemplified by the proximity fuze, were in production. Aircraft autopilots, low-noise communications, and fire-control systems became widely available. Most of us realized how far the techniques had moved only when, in postwar surveys, we observed the extent to which Allied efforts had outstripped those of the Germans and Japanese. I found, for example, that although the V-2 development rounds carried a radio telemetry system, the Peenemunde engineers had to rely principally upon tracking and recovery of the wreckage for performance and diagnostic information.

By the time I returned from my first trip in the fall of 1944 and began activating JPL's Remote Control Section (also responsible for test instrumentation, flight performance "reporting" or telemetering, and tracking), two rocket-vehicle project efforts were underway. The first, called Private, was intended to provide early integration and launching experience and to yield whatever applied-research values were possible from so simple a design. It consisted essentially of a solid-propellant Aerojet JATO unit with aerodynamic nose and fin assembly added; it was launched from a rail by a cluster of four solid-propellant rockets. Stabilized by fins after burnout of the booster, Private required no guidance system; there was no attempt to equip it with radio telemetry. The flight trajectory was deduced from radar and motion-picture data. On the last two rockets of the first series, Private A, in December 1944, we installed armored motion-picture cameras in the nose, looking sideways, to give information on the roll rate. Though battered by launch and impact, this rudimentary flight instrumentation did give the necessary data (Figure 1).

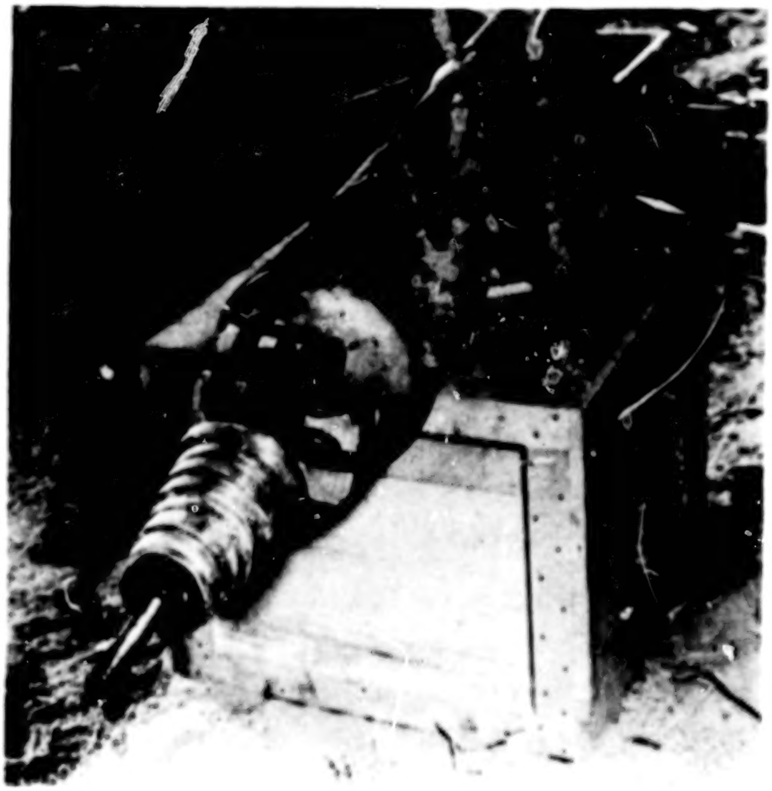


Fig. 1
Private A Rocket Test Vehicle, Instrumented
With Motion-Picture Camera

This first ORDCIT vehicle was tested in the California desert north of Barstow, not far from the present Goldstone Tracking Station, in December 1944. It gave valuable experience not only to the JPL team, but also to the group from Aberdeen Proving Ground's Ballistic Research Laboratory, who provided radar tracking. The next Private series involved a winged version of the missile, called Private F; this was tested at the Hueco Range of Fort Bliss, Texas, near the White Sands site then under construction. Private F carried no flight telemetry or guidance, but the eye and the camera were enough to reveal the corkscrew instability of its flight path (Figure 2).

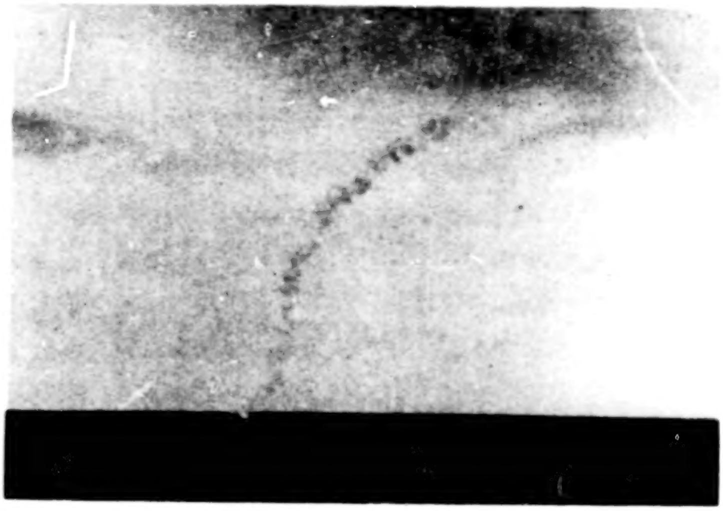


Fig. 2
Private F Flight Test, Hueco Range, Texas, April 1945
Showing Rolling Instability

Planning had begun for the "second type of long-range jet-propelled missile," named Corporal, before I had returned from the East Coast.[5] This was to be a guided vehicle, launched vertically, with a 20,000-lb-thrust rocket engine using aniline and red fuming nitric acid. It would be a large technical step up from the Private series and from prior art in rocket-engine design, aerodynamics and flight path, airframe, and most of all in telemetry and guidance. For several years this vehicle would be the focus of most of JPL's research--especially my own.

The emphasis in those years at JPL, in ORDCIT, and on these missile efforts was upon research. The Private and Corporal supported research and investigation, not vice versa, and although development of a military guided missile was the ultimate objective of our contractual work, the contract called for us rather to understand the problems of the system.[6] A summary of these research-support rockets is given in Table 1. A similar mandate prevailed in the propulsion studies: GALCIT Project No. 1 had studied the characteristics of rocket motors for aircraft propulsion and JATO, but the Aerojet Engineering Company (founded by Kármán, Malina, and associates) designed and produced the final hardware products. This applied-research impulse resulted from close institutional bonds between the scientists and government personnel, particularly Army Air Forces but including other armed services. These bonds had been built by Kármán in the field of Aeronautics

Table I.
JPL Rocket Research Vehicles of the 1940s

Dimensions	Private A 10-in. dia, 8 ft. long	Private F 10-in. dia, 8 ft. long, 3 ft. wings	Wac 12-in. dia, 12.5 ft. long	Corporal 30-in. dia, 40 ft. long	"Sergeant" [c] 15-in. diam.
Gross Weight	600 lb.	500 lb.	665 lb.	12,000 lb.	not applicable
Flight Profile	Ballistic from inclined launch, 10-mi range	Private A with lift for range increase	Vertical to 240,000 ft.	Ballistic from vertical launch, 60-mi range	Vertical (exceeding Wac)
Propulsion	Aerojet solid, 1000-lb thrust	Aerojet Solid, 1000-lb thrust	Aniline-RFNA engine, 1500-lb. thrust	Aniline-RFNA engine 20,000-lb. thrust	Polysulfide solid, approx. 6000-lb. thrust
Launch	From rail, with booster	32-ft. rail with booster [a]	102-ft. tower, with booster [b]	ground platform no booster	no booster
Guidance	free-flight	free-flight	free-flight	Pneumatic autopilot, radar-command	electronic autopilot
Flight Test Operations	Leach Springs, Calif., Dec. 1944, 24 rounds total	Hueco Range, Ft. Bliss, Tex. Apr. 1945; 17 rounds	White Sands, N.M., Oct. 1945 (Wac A, 10 rounds) Dec. 1946-47 (Wac B)	White Sands, N.M., May 1947-1950 (Corporal E, 5 rounds launched)	Not flight tested; static firings at Camp Pendleton, Calif Feb.1949-50;12 motors

[a] Private booster: four Army 4.5-in. solid rockets total thrust 22,000 lb for 0.18 sec.

[b] Wac booster; NDRC Tiny Tim ballistite solid rocket, thrust 50,000 lb for 0.6 sec.

[c] This "Sergeant" research vehicle should not be confused with the Sergeant guided missile—See Table II.

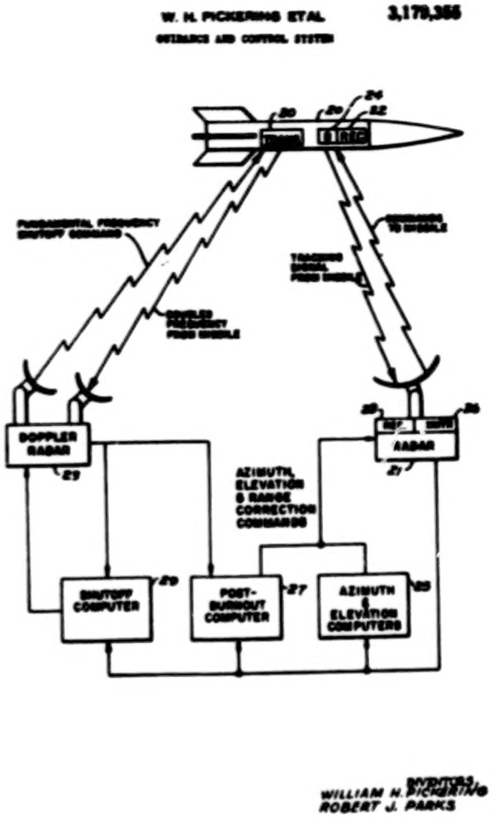
during the previous decade, and reflected the science-in-society tradition established by Robert A. Millikan and George E. Hale a generation earlier in World War I.

When ORDCIT began, two complementary principles were in operation: that of cooperation and mutual understanding between the academic scientists of Caltech, the military officers, officials of the government, and the working engineers of industry; and that of the autonomy of the technical expert and investigator. The resulting balance was also worked out on a grand scale in the nationwide wartime research mobilization led by Vannevar Bush. The autonomy principle was formalized and strengthened by Louis Dunn soon after he took over direction of the Laboratory in 1947: the broad Air Forces contract was modified slightly to recognize the right of the Institute, as well as the government, to approve the technical tasks proposed.[7] This independence was seldom exercised in fact, but it assured the integrity of the Laboratory as an impartial technical advisor.

In 1945 I prepared a preliminary analysis of guidance and telemetry problems for Corporal.[8] After reiterating the research mission of Corporal, I divided the flight profile into (1) vertical ascent, (2) declining-angle thrust phase, and (3) parabolic free flight, each with its particular control requirements. (We might note that the smaller test vehicles evaded such requirements by the nature of their launch modes and flight profiles.) Stabilization during the vertical ascent, by a gyro-controlled autopilot, and the programmed pitch turn were required by Corporal's mode of flight, and control of the burnout velocity and flight-path angle to provide the required target accuracy. I selected the radar-command mode for this guidance, whose elements are illustrated in Figure 3.

While the Corporal research vehicle was maturing on paper, Frank Malina conceived an interim scaled-down test vehicle which could also be used as a vertical sounding rocket--the climactic goal of the original Caltech student group. It was soon named Wac Corporal. Designed to carry a 25-lb instrument payload to 100,000 ft, it was to be unguided, boosted out of a 100-ft tower by a "Tiny Tim" solid rocket and propelled by a 1500-lb-thrust acid-aniline rocket engine (Figure 4). The first version, tested in the fall of 1945 at White Sands (and inaugurating that facility) carried a Signal Corps radiosonic package, but no missile telemetry; a later version, tested in December 1946 and the following Spring, carried a five-channel JPL-developed FM/FM telemetry system as well as a parachute recovery scheme which could return the instruments or even the whole missile undamaged for re-use (Figure 5).

The Wac Corporal was a triumphant program in many respects.[9] Outgrowing its R&D test-vehicle function, it offered for the first time the realistic role of scientific instrument carrier in a simple, relatively cheap form. It outperformed specifications, exceeding 200,000 ft altitude (a world record at the time). It brought forth a new design cycle for rocket engine and airframe. But most important here, its launch operations, involving the cooperation of JPL crews, the Aberdeen tracking team under L.A. Delsasso (which had also supported prior flight tests), and the White Sands range, were a valuable preparation for Corporal testing.


W. H. PICKERING ETAL
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GUIDANCE AND CONTROL SYSTEM
30
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24
22
FUNDAMENTAL FREQUENCY
SHUTOFF COMMAND
DOUBLED
FREQUENCY
FROM MISSILE
COMMANDS
TO MISSILE
TRACKING
SIGNAL
FROM MISSILE
DOPPLER
RADAR
28
26
RADAR
21
AZIMUTH,
ELEVATION
& RANGE
CORRECTION
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COMPUTER
27
AZIMUTH
&
ELEVATION
COMPUTERS
25
INVENTORS
WILLIAM H. PICKERING
ROBERT J. PARKS

Fig. 3
Guidance and Control System for Corporal
(From U.S. Patent 3,179,355)

I was out of the country during the first series of Wac tests in 1945; in company with Kármán, I was looking into the test instrumentation and electronics developments of Germany and Japan, where we found very little that could add to what the Laboratory was already doing. I participated in Wac B operations, however, and began an interesting association with the system problems of test-range operations and flight instrumentation. In the meantime, the Corporal vehicle moved deliberately but surely towards its first flight.

On May 22, 1947, Corporal E No. 1 rose from White Sands. Weighing almost six tons and stabilized by a pneumatic Sperry autopilot, the slim white rocket lifted off its flat stand, gradually pitched forward toward the target, and flew a ballistic curve to within two miles of its 62-mile target range (Figure 6). No precision guidance had been employed, though an experimental radio command was exercised successfully. A ten-channel FM/FM telemetry set (actually two of the Wac telemetry sets) returned measurements of

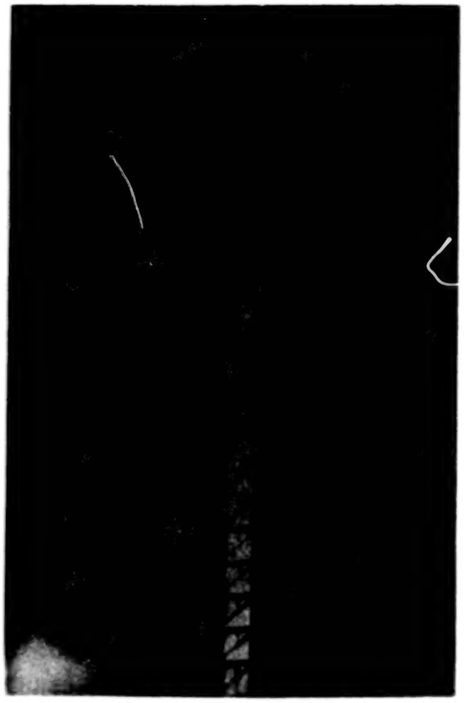

Fig. 4
WAC Corporal Launch, Just
At Booster Separation

guidance and propulsion-system parameters, and the trajectory was plotted from radar position and velocity measurements acquired by an active (flight transponder) tracking system (Figure 7).

Buoyed by this success, we prepared the next bird for launching eight weeks later. The second Corporal flight test, however, was a fiasco. Apparently the air-pressure regulator, which controlled the flow of propellants to the rocket engine, malfunctioned; the engine ignited, but with insufficient thrust, and burned on the stand for 90 seconds before the rocket was light enough to get off the ground. Then the missile rose, tipped over, headed for the sand, and proceeded to skitter through the desert underbrush under power until it blew up (Figure 8). A wag at the scene named it "the rabbit killer." The next two flight tests were better than this, but not so good as the first. The following two years were filled with bench experiments, redesigns, and ground testing. Certain essential features of the system remained to be flown, in particular the complete guidance system.[10]

In addition to the Corporal and its antecedents, which engaged elements of nearly every Laboratory section, there were several specialized rocket projects in this period which gathered a few—usually about two--research strands into a flight-test focus.

Fig. 5
WAC Nose Section After Recovery
(Author in Foreground)

The first of these jointly interested my telemetry group and some of H. J. Stewart's research analysts beginning in 1944; it was called RAFT, the rocket airfoil tester.

As the name implies, RAFT was designed to obtain aerodynamic information. At this time, supersonic wind tunnels were few and far between, and the transonic region was and still is difficult to simulate in a tunnel because of wall effects. The alternative is a free-flight vehicle, mounting the test airfoil and necessary instrumentation. The RAFT vehicle was simply a five-inch Navy ordnance rocket, with test section and instruments replacing the warhead (Figure 9). The need for telemetering the measurements gave my group the challenge of putting together a small, rugged, flight telemetry set and associated ground equipment; the test operation, conducted first in the Mojave desert site in early 1945, gave us valuable early experience in field work and data acquisition. These three-and five-channel sets were the first working rocket telemetry units in the United States. They operated on the FM/FM scheme, with the measured forces varying the frequency of a subcarrier oscillator, and several separate subcarrier signals frequency-modulating the RF carrier.[11] This scheme was subsequently carried over to Wac and Corporal.

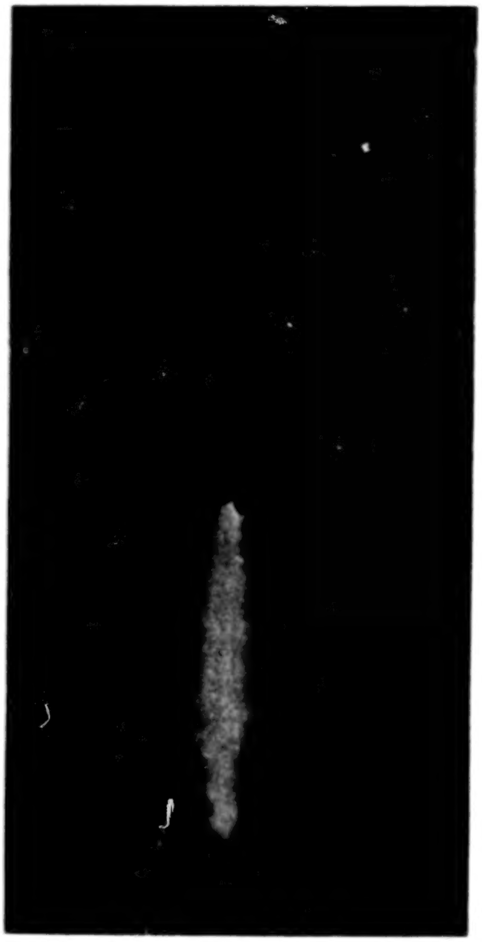

Fig. 6
First Corporal Launch, White Sands Missile
Range, New Mexico, May 22, 1947

Other special-purpose rocket vehicles supported research in solid propellants and high-acceleration, high-speed dynamics, A small vehicle unofficially called "Thunderbird," demonstrated the polysulfide composite-propellant, internal-burning star-grain solid motor in 1947. With an acceleration over 100g, it led to the Wac-scale solid-propellant research vehicle called Sergeant in 1948.

But the Sergeant sounding rocket, unrelated to the tactical missile of the same name, proved to be ahead of its time. It was inspired by calculations that indicated a solid-propellant rocket of the internal-burning-star design could deliver several times the payload of a liquid-propelled V-2 type of similar weight. The motor chamber walls were to be very thin because the propellant, burning from within, would help contain heat and pressure. An autopilot design effort was begun, and static tests of the motor,

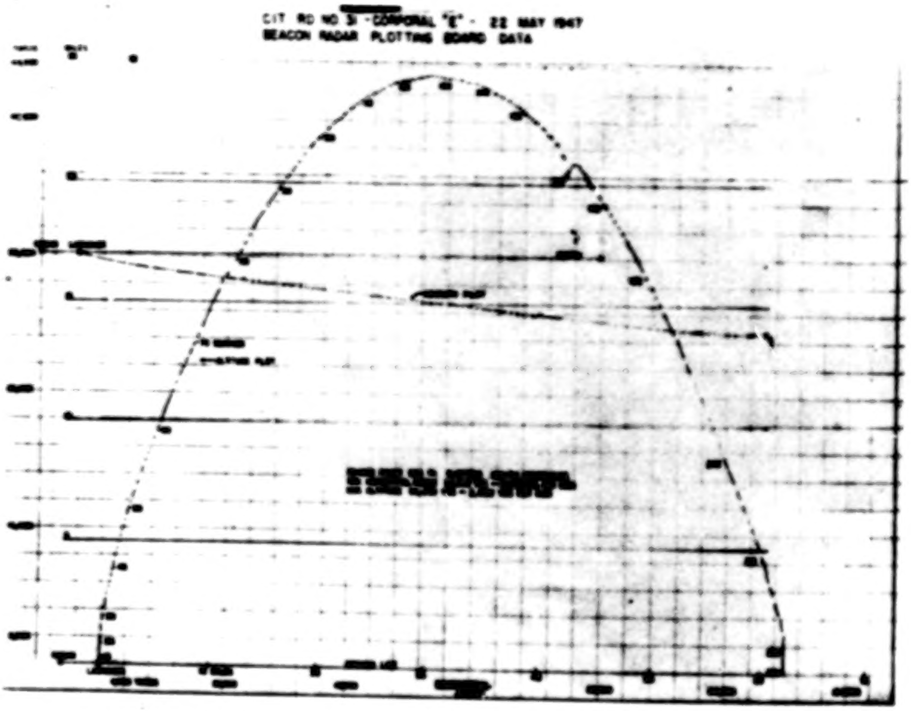

Fig. 7
Corporal E-1 Trajectory From Radar Data

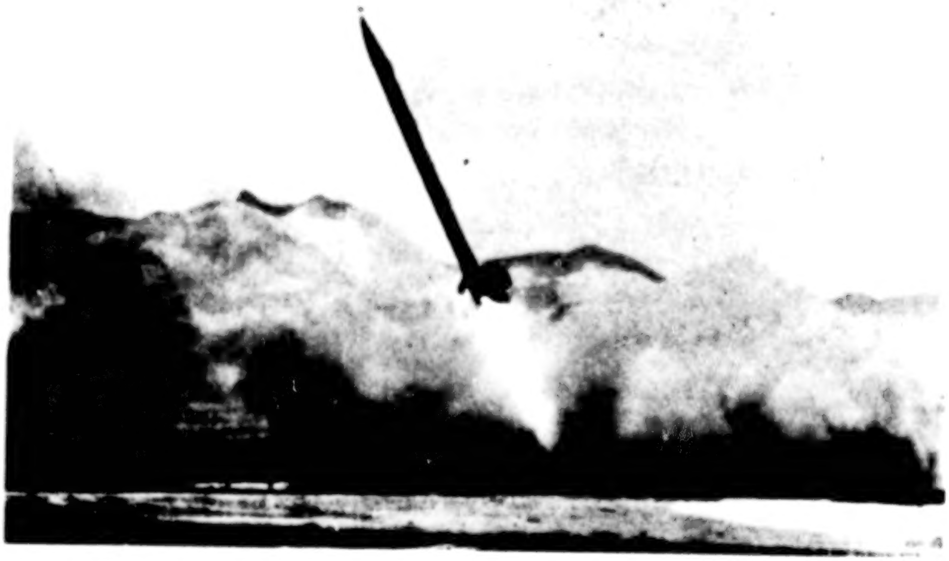

Fig. 8
Corporal E-2 Launch

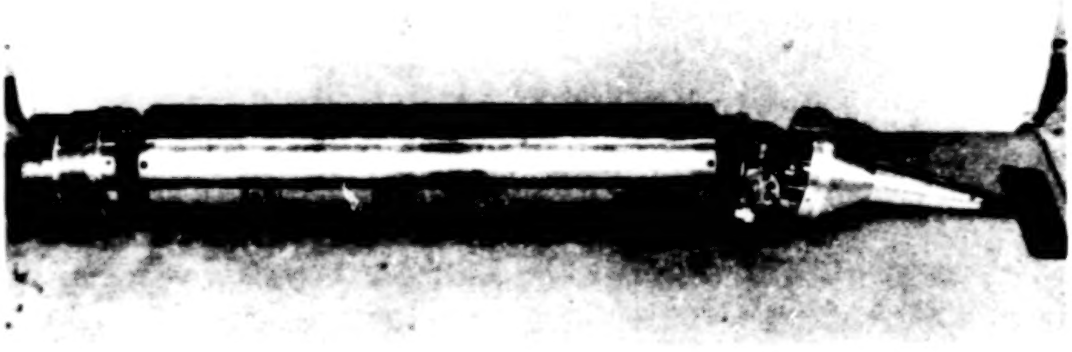

Fig. 9
Rocket Airfoil Tester (Without Rocket Motor) Showing Mounted
Airfoil, Instrumentation, Telemetry Compartment

weighing 1300 lbs and delivering about 6000 lbs thrust for more than 30 seconds, were conducted (Figure 10). Difficulties with solid rocket, manifested in the rupture of the thin-wall case, coincided with a change in the JPL mission and an acceleration and

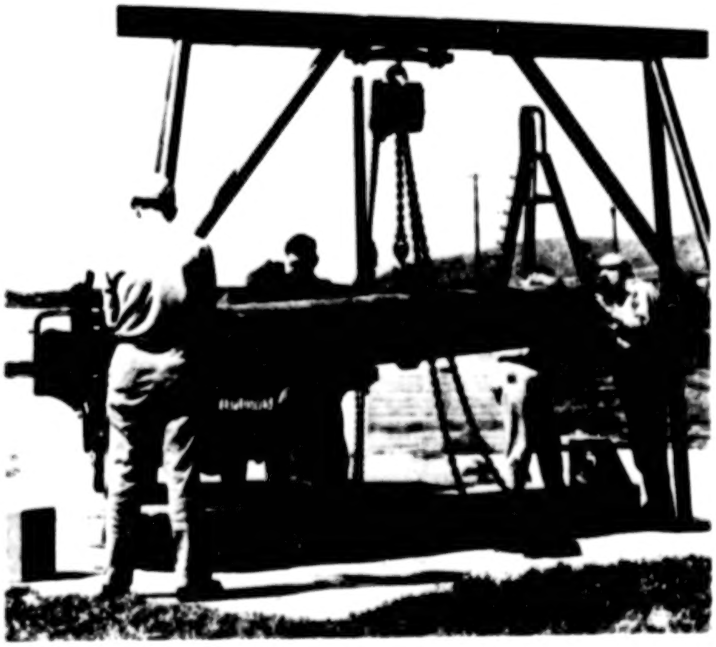

Fig. 10
Experimental Solid-Rocket Motor, "Sergeant," Preparing
For Static Test, Camp Pendleton, Calif., 1949

expansion of Corporal development; the Sergeant project was suspended. The electronic autopilot was adopted for the Corporal missile while the solid-propellant engineers took their problems back to the laboratory and test stand for more investigation. Further development was undertaken by the Thiokol Chemical Corporation. The ultimate heritage of this early "Sergeant" powerplant was the reliable solid rocket, used in large scale in the Sergeant and other military missiles, and in clustered miniatures to launch the first Explorer satellites.[12]

Other projects of an interagency character also focussed JPL efforts in this period. They did not center upon rocket vehicles but rather upon large-scale test facilities. The first was the Supersonic Wind Tunnel, which grew in part from the ram jet propulsion project but mostly from the needs of the designers of the long-range rocket for supersonic test data. The wide range and high value of Mach numbers under which the rocket models would need to be tested went beyond the capability of any available facility. A general need for such facilities could be foreseen, and the talents of Caltach--in particular the brilliant aeronautical engineer Allen Puckett--were ready and willing to fill this gap. Accordingly, the Army and Air Force authorized JPL to proceed. A flexible-throat wind-tunnel of 12-in test section was completed in 1947, and a 20-in tunnel in 1950. This enterprise supported not only JPL rocket design and research programs, but also a large group of rocket and supersonic aircraft developers and research laboratories under the aegis of the two armed service sponsors. At one time the wind tunnel took up about one-quarter of the Laboratory's effort, and it remains in operation to this day.[13]

The second interagency effort was more directly concerned with the rocket projects--ours and others--and involved my own efforts as well as the Laboratory in its consultative role. The primary external need posed by the long-range rocket projectile was a well-instrumented flight-test facility. We began to use and improve the White Sands range in 1945, inaugurating it with the first Wac Corporal in the fall of that year. We learned a great deal about the instrumentation and operational requirements of rocket testing in those early years, and, when the Navy developed their Point Mugu test range (beginning in late 1946), I performed a number of studies to help them set up their instrumentation system.[14] We shared what we had learned more informally with the other users of White Sands, which included the V-2 scientific and engineering program, the Nike antiaircraft development, and the Navy's Project Viking.[+]

This interchange of information, and the standardization of practice which followed, was coordinated by the Research and Development Board of the Department of Defense. One of the committees operated by this board, chaired in succession by Edmond C. Buckley and myself, was concerned with test range instrumentation: tracking, telemetry, and command control. We wrote standards for an FM/FM telemetry system, expecting this to be an

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[+] See Milton Rosen, "The Viking Rocket: A Memoir," in this volume--Ed.

interim stage pending the adoption of pulse telemetry systems. To the surprise of a good many people, FM/FM proved quite durable, and remained in good use much longer than expected. The successor to our committee, the Inter-Range Instrumentation Group, maintained our standards in terms of subcarrier channels, but in later years the field of telemetry modulation gradually opened up to a great variety of schemes.

By mid-1949, then, the rocket- and missile-related research of the Laboratory was focused principally in the Corporal as a test vehicle for the technology of the liquid-propellant guided missile, with smaller solid-propellant efforts gathered around the Sergeant research vehicle, and aerodynamic research and testing in the wind-tunnel. The still-growing electronics activity was engaged in guidance, telemetry, and tracking for the missiles, and in instrumentation systems for the wind tunnel and the rocket firing ranges. My own interest was primarily engaged by Corporal's problems, though I was concerned with ground instrumentation systems and the distant potential of the rocket for upper-atmosphere research.

III. THE GUIDED MISSILE SYSTEM

Nothing in nature remains fixed, and the swinging pendulum of research and development under which we had worked on Corporal swung rapidly forward in the fall of 1949. The experimental rocket vehicle which we used as a device to study guidance, missile aerodynamics, and the like, was transformed into a guided missile under development.

Under the pressure of the changing world situation, and with the guided-missile program now five years old, Army Ordnance reviewed the prospect of obtaining an accurate and usable weapons system from our ongoing study effort. This was, after all, their objective. Their program had moved forward on two fronts: ORDCIT on an applied-research basis, and Project Hermes building from the captured remains of the V-2 development as an industrially-based technological program at the General Electric Company. With four flight tests accomplished, one of them highly successful, Corporal was evidently making good progress towards Ordnance's goal, and Colonel (later Major General) Holger Toftoy of the Ordnance Missiles and Rockets Branch, asked Louis Dunn, JPL's Director, to determine whether it could be converted to a weapons system.

Louis Dunn, who had been generally responsible for the Corporal program since 1945, reasoned that the major technical step was the provision of an accurate guidance system. He accordingly brought me into the inquiry, and we gave the matter much thought before deciding in effect, "let's give it a go." In September 1949 we travelled East to confer with the Colonel Toftoy at the Pentagon. He talked to us about the need for a demonstrably field-worthy guided missile, the limited prospect for achieving this soon, and the Ordnance Department's wish that JPL move Corporal rapidly in this direction. I was much impressed by the faith of this professional soldier in the industrial-development capability of our Laboratory, considering that we were so largely research-oriented and had no experience of this kind. I believed we could solve the technical problems of the guidance system, however, and Louis was confident that we could handle the industrial-engineering transition. Thus began the Corporal guided-missile development project, for which Louis asked me to take responsibility. JPL began to change from a research organization to a duel-purpose, research and hardware-development team. It really was as simple as that: we said we could do it, and Toftoy told us to go ahead.

Things began to move. In April 1950 the Army Ordnance Guided Missile Center was activated at Redstone Arsenal, Huntsville, Alabama. Although at first the Department of Defense disapproved the Army's request to list Corporal as a weapons system development, in October 1950 they established the Office of Director of Guided Missiles at a high department level. In the same month JPL was issued a contract for the new Corporal program.[15]

This shift in the Laboratory's objective was accompanied by a more gradual change in the organization, in part to reflect the recognition of a dichotomy between research and development engineering. In the early years there had been a relatively free mixing of researchers and rocket-builders, and it had often been possible and appropriate for one man to follow his work through research to hardware development. By late 1944, however, we had separate guidance and electronics-research activities. In 1946, the many technical sections were clustered into four divisions; in Dunn's reorganization of 1950 two of these emerged as research oriented, contrasted with my division, now called Guided Missile Electronics, and the one headed by Paul Meeks, Guided Missile Engineering.

I was not altogether in agreement with this dichotomy, partly because my own interests and the technical discipline of electronics are strongly rooted on both sides of the fence. The electronics activity at JPL had a dual role from the start: the immediate development of test instrumentation systems with long-term guidance research, or, in the later period, missile and test-range electronics together with communications theory. Thus, my inclination was more toward organizing on the basis of technical disciplines, and lowering the barriers between research and practical operational development. In 1954, after I was named Director of the Laboratory, additional growth in form and function called for another reorganization of the Laboratory, and we moved toward somewhat greater integration of research and engineering, with three departments divided essentially upon discipline lines. By the time of Explorer 1, the three Department Chiefs (respectively Frank Goddard, Jack Froehlich, and Bob Parks) had additional responsibilities in the areas of research (including the wind tunnel operations), the space project, and the missile project, respectively, though these three activities each drew on the technical resources of all three departments.

But in 1950 when it formally became a candidate guided-missile system, Corporal was still evolving slowly as a research vehicle. The propulsion system, mature in principle, was still developing in terms of hardware as the designers probed the dimensions of weight, material, and configuration. The autopilot was at a similar stage, having progressed from the hydraulic design used in aircraft to a pneumatic one, whose vulnerability to the heat and vibration environment of the missile was demonstrated in the fourth flight test. Long-range guidance to provide accuracy at the target was still maturing in principle; test instrumentation, radio telemetry, and radar tracking were essentially ready to meet the needs of the development program.[16]

The most critical technical problem was long-range accuracy. Corporal had to reach a target only a few hundred meters across, about 110 kilometers from its launch site, after flying a course which left the atmosphere and re-entered it. The broad principles of radar-command guidance were those I had worked out in 1945, and have mentioned above; the practical realization of equipment which would perform correctly and could be mass-produced was a distinct challange.

This was complicated, in turn, by two "strategic" conditions. First, none of us knew quite how accurate or reliable such a guidance system ought or needed to be; and second, the Army wanted one quickly. As a consequence of the first condition, the requirements for Corporal tended to evolve with the system, as the theoreticians, the engineers, and the military planners worked along and learned their way, and also as the Corporal test program fed information back to them. The second condition made it desirable that we work toward an interim solution rather than ultimate perfection. The Corporal system thus emerged as a hybrid, or lash-up, based on existing equipment or designs: A modified Corporal E rocket (Figure 11), an all-electronic autopilot derived from the Sergeant test-vehicle project, modified Signal Corps 584 radar, and a doppler link for velocity measurement. For ground handling vehicles, we modified the designs of construction and agricultural machines, among others. It was implicit in this approach that Corporal would be succeeded by a new and better system, probably in the 1960s.

As ultimately developed, the Corporal guidance system conformed essentially to my plan of 1945: the inertial autopilot stabilized it through launch and continued in operation, but it was augmented in later phases of flight by overriding commands from the more precise ground computer, operating from radar and telemetered missile gyro data. Range control was maintained by shutting off the rocket engine when the proper velocity had been attained; this was sensed by the separate doppler link, and executed by a very precise valve system actuated on radio command from the ground computer.[17] A measure of the accuracy of this early radio guidance is that within a few years we were able to find errors in the survey maps of White Sands.

In the Corporal E flight tests, we had early discovered guidance and control equipment problems arising from the rugged environment of a rocket in flight: the early autopilot was placed next to the rocket engine, surrounded by heat and vibration. Mountings and structures often would amplify the vibration by resonance, and we learned a great

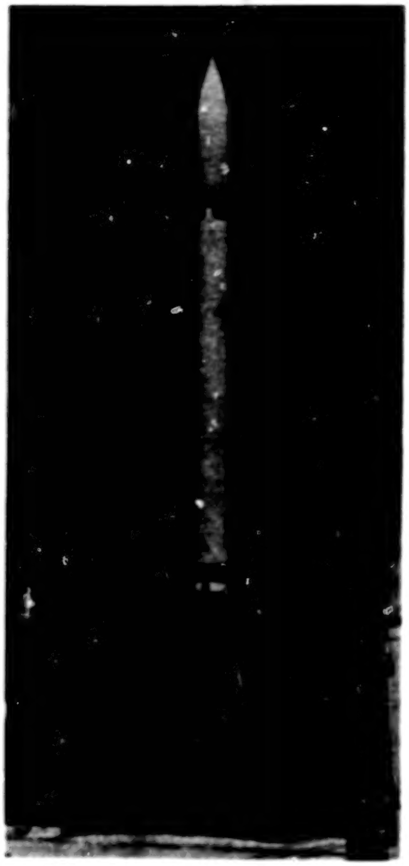

Fig. 11
Early Weapons System Version of Corporal, With Handling Equipment, White Sands Missile Range

deal about environmental testing and the design of sturdy and reliable equipment, not only for the missile itself but for the complex ground equipment which must operate in the field as well. This special technology was to have important implications for space exploration, of course.

By 1953 the Corporal system was ready to be tested in tactical conditions. A sketch of the launch area in the field is given in Figure 12, and a natural view is shown in Figure 13. Two industrial firms, the Firestone Tire and Rubber Company for the missile, and Gilfillan Brothers, Inc., for the guidance system, had begun limited production. We had already launched some forty missiles at White Sands in the course of experimental and development testing. Accordingly, the Army began a long series of field test firings, while we continued development flights at a rate which reached one a week over an extended period. A small JPL contingent had taken up permanent residence at White Sands by this time, and a much larger group spent a great deal of time there. Although JPL involvement

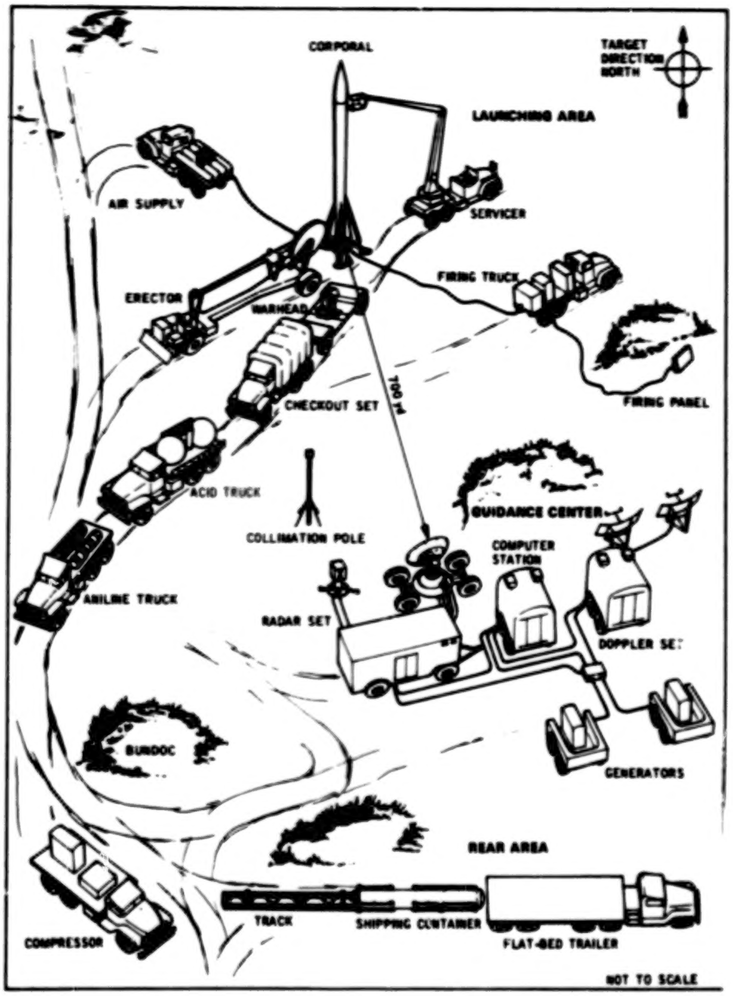
CORPORAL
TARGET DIRECTION NORTH
LAUNCHING AREA
AIR SUPPLY
SERVICER
FIRING TRUCK
ERECTOR
WARHEAD
700 yd
CHECKOUT SET
FIRING PANEL
ACID TRUCK
GUIDANCE CENTER
COLLIMATION POLE
COMPUTER STATION
ANILINE TRUCK
RADAR SET
DOPPLER SET
BUNDOC
GENERATORS
REAR AREA
TRACK
SHIPPING CONTAINER
FLAT-BED TRAILER
COMPRESSOR
NOT TO SCALE

Fig. 12
Corporal Tactical Field-Test Diagram

in Corporal continued at a vigorous pace, from the point of view of statistical testing, technical advice and training for the military, cooperation with the industrial production engineers, and advanced development, the development was essentially completed.

At about this time, Corporal's successor entered the picture. In January 1953, Army Ordnance asked us to investigate the feasibility of an improved solid-propellant missile capable of taking over the Corporal mission. Our study revealed that considerable improvement in accuracy, reliability, and ease of handling was possible with such a vehicle, provided an improved radio-inertial guidance system was employed. The benefits of replacing the complex liquid-propellant powerplant with a one-piece solid motor were

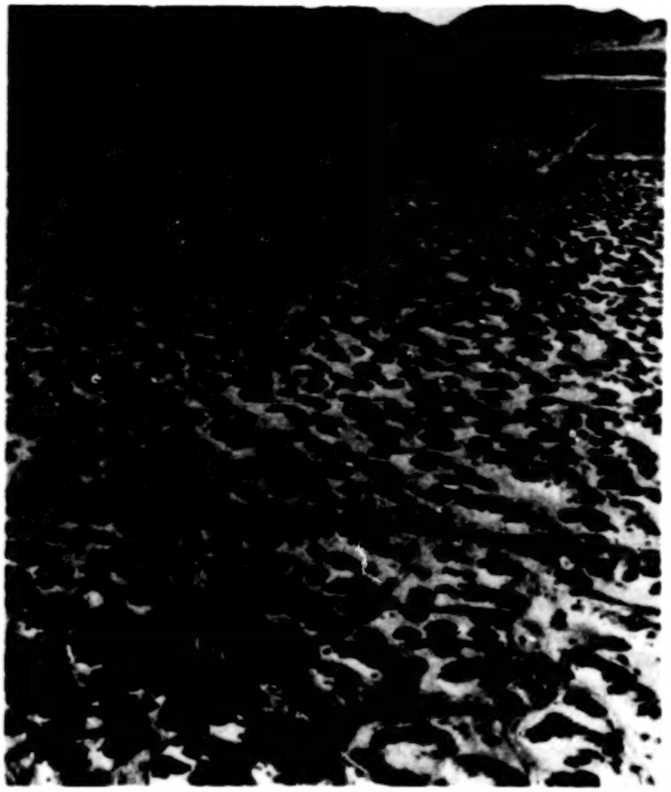

Fig. 13
Corporal Tactical Field-Test Arrangement at White Sands

obvious, especially now that the motor evolved from the Sergeant test vehicle of 1948-49 via the Hermes RV-A-10, had been perfected. But this simple and easily-handled propulsion system offered one large problem to the guidance designer--it could not be simply shut off with the closing of a valve. In addition, it provided a challenge to the electronics packagers in the form of a new and stronger vibration environment. Still another problem faced the guidance engineers: the missile had to be immune, or nearly so, to radio interference and jamming. The obvious solution to this was to remove the ground link and program the missile to control itself internally, using inertial data. In 1953 this solution appeared unsatisfactory from the point of view of costs, reliability and accuracy. As a result, we worked on an essentially noise-free and jam-proof communications link for the radio guidance system.

The benefits of all we had learned in the Corporal development were freely available to the new Sergeant missile program, partly through a continuity of people between the projects. Principal among these was Robert J. Parks, a long-time associate and former student of mine, who had succeeded me as chief of the Guidance and Control Section and of the Guidance Research Division, and became Sergeant Project Manager. The communications links were simplified to one up and one down from the missile, with command, telemetry, and doppler tracking all gathered into one looped circuit. Transistorized electronics were a part of the Sergeant design from the start, while Corporal telemetry had had, for example, to be "updated" with such circuits. New equipment packaging, in which the subsystem units took the form of nearly solid blocks, in which the chassis were hollowed-out shells rather than flimsy internal skeletons, reduced our worries about vibration. Automatic checkout equipment rapidly verified the readiness of all circuits for launch.

The major guidance problem, velocity control, was solved when the JPL system design group developed a set of aerodynamic drag brakes (Figure 14). Operating rather

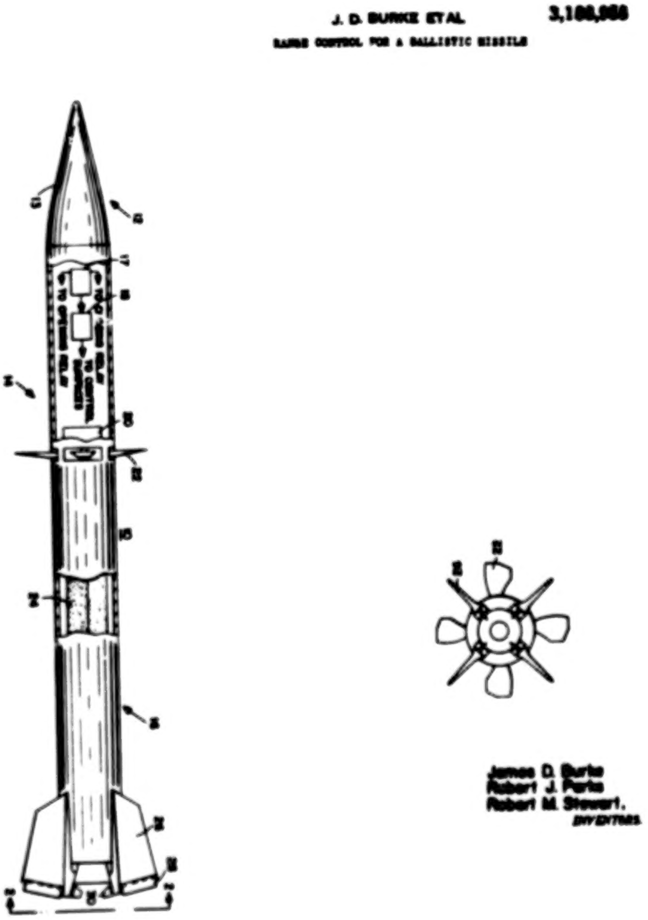
J. D. BURKE ETAL
3,188,958
RANGE CONTROL FOR A BALLISTIC MISSILE
James D. Burke
Robert J. Parks
Robert M. Stewart,
INVENTORS

Fig. 14
Sergeant Range-Control System
(From U.S. Patent 3,188,958)

like the blades of a camera shutter, the brakes reduced the velocity to the required value. Flight-tested, the design proved as simple and precise as the shutoff valves which performed the corresponding function on the liquid propellant Corporal.[18]

Yet a fundamental debate still remained on the central issue of second-generation missile guidance. Those of us with Corporal experience advocated continuing the principle of keeping the major computing function on the ground, and using the missile-ground link as the accurate reference-measuring system. We felt that all-inertial guidance schemes, then being developed for early ICBM programs, would be too massive and expensive for Sergeant. But Army Ordnance and the field forces favored the invulnerability and simplicity of this mode, and authorized parallel developments, with support from industrial contractors, of all-inertial and radio-inertial guidance systems for Sergeant. In a relatively short time, Bob Parks, with John Scull and other JPL Colleagues, came up with a highly acceptable small stable platform for the Sergeant. Although the advanced ground-based guidance derived from Corporal was somewhat more accurate, it could not match the inertial design for field simplicity and freedom from radio interference. In 1955, the inertial system was formally selected.

The Sergeant missile system development went forward at a more rapid pace than Corporal, partly because of the simplicity and excellent performance of its powerplant, but mostly because both the engineers and soldiers were building upon the Corporal experience. Less than three dozen Sergeants were test flown at White Sands (Figure 15), compared with over one hundred Corporals; both JPL and the Army had learned how to handle guided missiles and test firings smoothly, and had flown certain crucial Sergeant experiments as passengers aboard earlier Corporals. Finally, Corporal had taught us how best to use industrial collaboration. The engineering firm which would build Sergeant, a new division of the old Sperry Gyroscope Company I had visited on my first trip out of JPL a decade before, joined as co-contractor during Sergeant's development.

One minor missile-system project of the early 1950s deserves mention at this point, for it served as a solid-propellant research focus and a case study in missile development. Project Loki began as an attempt to adapt the World War II German Taifun antiaircraft rocket technology. This rather small unguided projectile was developed for some time using the liquid-propellant design of the original before the Army asked us to study it in the solid-propellant mode early in 1951. The rocket booster was about six feet long, three inches in diameter, and joined to a ballistic dart about the size of the handle of a broom. It was spin-stabilized. For R&D purposes, the dart contained a tracking beacon and telemetry set. Designed for targets at 40,000 ft. altitude, the missile reached an acceleration of over 100g's after being launched from a long tube (Figure 16). Although we could build individual rounds precisely enough to achieve the necessary accuracy, the mass-produced version fell somewhat outside the target circle. About this time a military analysis showed that a different approach—guided ascent, exemplified by

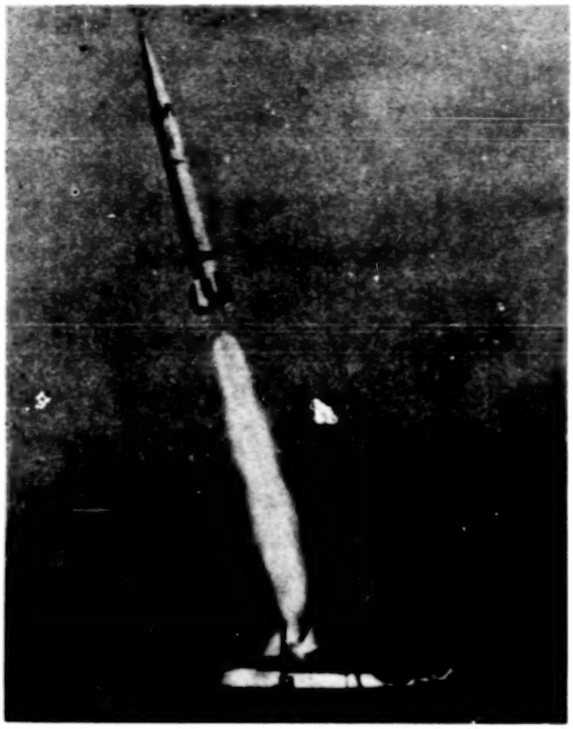

Fig. 15
Sergeant Launch, White Sands

Fig. 16
Loki Rocket in Test at White Sands, About 1952

Nike--was strategically superior to the Loki unguided-volley mode, and the project was terminated. However, many of the manufactured rockets were later used as small-scale sounding rockets and test-vehicle stages, a role in which they served with distinction.

While the laboratory appeared to enjoy a greater freedom of research in the earlier research-dominated period of rocket and guided-missile experimentation than in the Corporal-Sergeant missile engineering phase of the 1950s (see Table 2), the difference is partly illusory. For one thing, JPL was growing at a rapid rate throughout this period, and the expansion of missile development was not at the expense of reserach activity; in fact the two components grew together. For another, missile development produced new classes of research problems, of which elastic properties and telecommunications are two important examples. The ultimate positive effect of missile development on applied science was almost a case of serendipity rather than technological determinism, and represents the major theme of my memoir: the missile contributed much of the technology for, as well as the impulse towards, the practical science of astronautics.

It is only with hindsight that we can see the embryo of the spacecraft within the body of guided-missile developments in the early 1950s. At the time, in a project sense, we were oriented toward the practical problems at hand, even though the advances in technology had far-ranging implications, some of them as obvious as rocket propulsion and others as subtle as ground-based guidance. In addition, the practice in system integration and environmental testing that the missile projects afforded us would simplify the development of space systems. But throughout this period, from the inception of the Corporal to the termination of the Loki, the seeds of space exploration were germinating; in the middle of the 1950s they began to grow. We at JPL became involved in the proposal, and then in the development, of a space vehicle.

IV. TOWARD SPACE

JPL's formal activities directed toward space exploration began almost immediately following World War II. Frank Malina has described the origin and early success of Wac Corporal as a sounding rocket,[20] and I have alluded to its role in the Corporal technology. At about the time the first Wac vehicles were reaching toward "extreme altitude," JPL received a U.S. Navy contract to study the problem of orbiting a satellite. This activity had its origins in the Navy Bureau of Aeronautics in mid-1945, where the satellite mission, inspired by review of the V-2 development, was studied and considered for scientific, communications, and cartographic roles. The launch mode in this study was a single-stage rocket using hydrogen and oxygen; Robert Haviland and Harvey Hall were its most enthusiastic Champions.[21] JPL's involvement took the form of a technical review and confirmation of the Navy's calculations, and was conducted by the Research Analysis Section under Homer J. Stewart's direction. Stewart and his colleagues concluded that the

Table II
JPL Guided-Missile Project Activities

Name and Function	JPL Responsibility	Test Flights	Remarks
Corporal: radio-guided, surface-to-surface liquid-propellant ballistic missile	Complete missile system, incl. ground equipment and tech. training	Corporal E, 1947-50 (sec. Table I) flight 10, October 1951 Flight 20, May 1952 " 40, Dec. 1952 " 70, Jan. 1954 " 100, Spring 1955	Four flights in 1954-55 supported Sergeant guidance research; some flights in 1954 carried atmospheric research instruments for Ballistic Research Laboratory. In service, U.S. Army, from 1954; JPL development ceased, 1956.
Sergeant, inertially-guided surface-to-surface solid-propelled ballistic missile	Guided missile system.	Flight 1, Jan. 1956 Flight 10, Mar. 1958 Flight 20, Mar. 1959 Flight 29, Mar. 1960	Sperry served as co-contractor from March 1956. In service, U.S. Army, from 1963. JPL development ceased, 1960.
Loki, unguided solid-propellant antiaircraft missile	Rocket booster development and analysis only	1757 tests, White Sands, 1951-1955	Statistical data acquired, probes instrumented for tracking, spin rate, and temperature or pressure. Adapted as sounding and test vehicle.
Jupiter Project Support	Backup radio guidance	Three tests in 1957 on Corporal missiles.	"Codorac" system, noise-modulated, 1956-1957.
	Re-entry Test Vehicle support.	RTV-1, Sept. 1956 RTV-2, May 1957 RTV-3, Aug. 1957, at Cape Canaveral	High-speed stages (scale model Sergeant motors) and Microlock communications. Modified for satellite launch,Explorer 1

single-stage mode was feasible but marginal for a successful satellite mission, while multiple rocket stages would be preferable.[22] With a somewhat adverse technical judgement, and the difficulty of finding a proper military mission for the project, these early satellite studies gradually faded away, but the potential of military rocketry in the service of experimental science did not.

About sixty V-2 rockets had been brought from Europe to the U.S., and test-firings of these missiles began at White Sands in 1946. The opportunity to fly scientific payloads in these big birds was quickly seized, and an interagency group, which soon became the Upper Atmosphere Rocket Research Panel under James Van Allen's leadership, began to coordinate this work. I served on this panel, which eventually became the Rocket and Satellite Research Panel during the IGY. The Jet Propulsion Laboratory also took an early hand in the promotion and diffusion of upper-atmosphere and "space" science. In mid-March 1946 we organized a symposium on Guided Missiles and The Upper Atmosphere at Caltech, gathering scientists and engineers from university, industrial, and government laboratories to discuss high-speed aerodynamics, missile launching, the physics of the upper atmosphere, and problems in combustion and gas dynamics.[23]

This symposium, which heard thirty-six papers plus a supplementary discussion on upper-air experiments, was a prime example of the mixing process I have outlined. First of all it was an opportunity for many of those engaged in rocket development and its applied sciences, internal and external, to compare notes across a technical field from smokeless powder to super-aerodynamics—probably for the first time on such a scale. Second, and perhaps more important, it brought this broad population of rocket technologists into contact with an equally broad spectrum of potential scientific users, from the astronomer to the meteorologist. As an institutional mixer, the symposium was equally effective: JPL, Caltech, the University of California, and the R&D groups of Southern California's aircraft industry were strongly represented, while the eastern universities, various armed-service research groups, and at least one eastern industrial laboratory were also in evidence. The lines of communication represented and reinforced at this meeting, and maintained by the Upper Atmosphere panel, continued to be effective in the next decade of aerospace growth in the United States.

The value of the rocket to the kinds of experimenters present at this symposium, and the degree of organizing and planning work remaining to be done, are indicated in a publication of mine written in 1947. I wrote: ". . . it would appear that the design of the rockets is not adequately coordinated with the physical research programs to which they are intended to apply." The same conclusion was expressed in action with the development of the Wac-based Aerobee by the Applied Physics Laboratory of Johns Hopkins, 1946-1949 (under Navy sponsorship), and of the Naval Research Laboratory's Viking (1947-1955). My analysis matched desirable research programs and their necessary observational conditions against two representative available rockets, Wac and V-2, the research conditions they could provide, and the techniques they represented—especially in telemetry, instrument recovery, and the like. I suggested that extensive cosmic-radiation studies be deferred "until a satellite rocket can be produced," but I did not venture to predict that a period of ten years and seven months would be enough.

The difficulty with the sounding rocket for most atmospheric and space science, was the short life of the mission. For meteorlogical investigations, balloon-borne instrumentation was superior. The Wac, for example, flashed through the strata of interest too rapidly for sensors to respond; for upper-atmosphere physics and astrophysics, the duration at trajectory's peak, and our ability then to control the vehicle's attitude for instrument pointing, was marginal. Parachute recovery of the scientific payloads was considered rather as a means of supporting the use of spectral and photographic instruments than of remaining at altitudes of interest, for the latter were too high for a parachute to have helped. Finally, I considered the question of payload weight distribution, and it is interesting that my weight estimates for flight electronics equipment were approximately those used in Ranger and Mariner spacecraft in the early 1960s, though of course the development of microcircuits and the great increase in complexity and power have balanced each other in this case. The entire payload weight of Explorer 1 totalled less than the 20-lb figure I had allocated for telemetering alone for the research rocket.

Meantime, in the late 1940s, the two "available" high-altitude rockets came together for the next step towards space exploration. During the early Wac launchings, Holger Toftoy, Frank Malina, and others discussed combining V-2 and Wac as a two-stage test vehicle. This became the Bumper project in which JPL joined the General Electric Hermes activity, providing Wac B rockets modified for spin-stabilized launch at the peak of the V-2's trajectory (Figure 17). The high-altitude Bumper-Wac flights, which achieved a record altitude of 250 miles on February 24, 1949, did not return data from a scientific instrument payload. However they demonstrated the feasibility of rocket staging, and in particular that of joining two existing vehicles to produce a third. The later Bumper-Wac launches were devoted to the study of another technical problem, aerodynamics at high speed, and thus did not relate to space science.

Project Orbiter, which grew into a competitive proposal for a U.S. Satellite launch during the International Geophysical Year, resembled the Bumper vehicle in some system respects. That is, existing hardware was to be brought together for a new mission, supported by a multiple-agency team growing from roots set in Peenemunde and Pasadena, with a first emphasis on flight demonstration and such simplifying compromises as spin stabilization for the upper stages.

The Orbiter idea originated in mid-1954 at meeting of satellite advocates from the Office of Naval Research, the American Rocket Society, the astrophysics community, and (in the person of Wernher von Braun) the Army Ordnance rocket program. Those in attendance

Fig. 17
Bumper-WAC Launch

proposed to use a Redstone missile (descendant of V-2) as first stage, clusters of JPL-developed Loki solid rockets as upper stages, and a lightweight, inactive satellite payload. During the next year, this proposed fusion of V-2 technology, JPL rocket technology, and what was still designated as upper-atmosphere experimental science went through many stages of study and debate, in which JPL participated, until in 1955 it was finally rejected by the Defense Department in favor of the Naval Research Laboratory's Vanguard Program.[25]

We were asked to comment on von Braun's Redstone-Loki Orbiter proposal in November 1954. Homer J. Stewart's review, completed the following April, suggested replacing the Loki rockets in the upper-stage clusters with sub-scale Sergeant motors, which we made and used for a variety of tests in the Sergeant missile project. A typical cluster of this type is shown in Figure 18. A more elaborate study of this alternative was completed and released in July 1955.[26]

Up to this point, like the consideration of the long-range rocket projectile a decade before, the satellite proposal had been dominated by the rocket-propulsion function, by mechanical-engineering considerations, and was essentially devoid of instrumentation

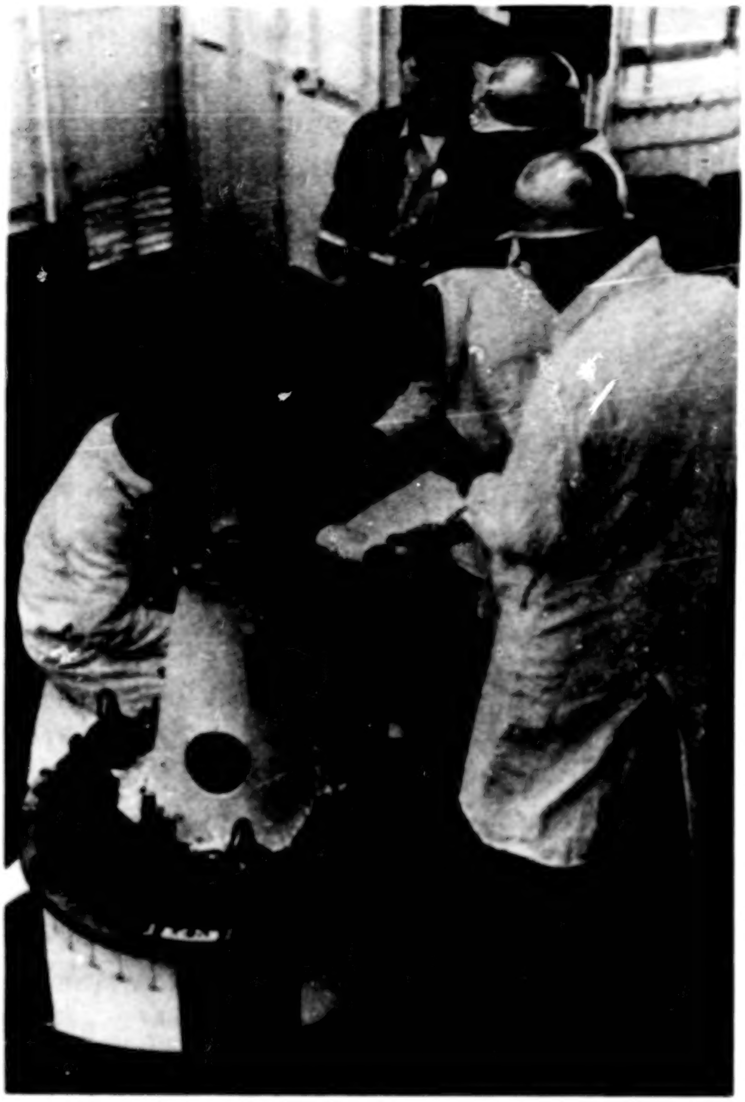

Fig. 18
Cluster of 6/31 Scale Sergeant Rockets Used as Upper Stages in
Re-entry Test Vehicle and Explorer Satellite Launching Vehicle

planning. Indeed, the original scientific plan had called for optical tracking of an inert satellite, a technique which offered some useful experiments and remained a supplementary method in the Vanguard project. Further, Orbiter's lack of stress on instrumentation, compared with the Naval Research Laboratory's thorough study of instrumenting the Vanguard satellite, was very influential in the ultimate decision in favor of the latter project.[27]

Before that decision became final, however, we set to work to redress the balance. Instrumenting the Ordnance-proposed satellite was not impossible, or even technically difficult. Rather, as in the case of radio telemetry for the Private rocket vehicles, or guidance for the early Corporal, it had been a second-order problem, and its consideration had been deferred. But it proved quite feasible to adapt tracking and communication techniques from practice and research in missile guidance to the satellite. The initial study was part of the revised Orbiter proposal offered when the Vanguard decision was reconsidered in August and September 1955; a more extensive report, introducing the name "Microlock," appeared in April 1956.[28]

These satellite studies were able to draw upon our applied research in guidance systems and our guided-missile development work in distinct and valuable ways. First, the technical basis of the Microlock system lay in information-theory and radio-frequency research originally related to jamming and radio-noise problems in missile guidance. Such research related equally to the need for tracking and communicating with a small, low-power space probe. Second, the vibration and acceleration environments expected for satellite equipment were comparable to those observed in missile testing, especially in the Loki missile development. Third, many of the problems to be considered in the satellite study were interdisciplinary and intersystem in the same way that problems of guided-missile development cross these boundaries. It should be noted that Microlock was a one-way tracking and communications system, having, unlike missile guidance, no up-link to send commands to the flight vehicle. The system design is given in Figure 19, and the portable ground site, with interferometric antenna array for satellite use, appears in Figure 20.

The original Microlock study and development had been oriented entirely toward the Army Ordnance proposal for an earth satellite. However, notwithstanding the improvements and continued arguments by Army representatives on its merits, the proposal was still not accepted by the U.S. Government, and Project Vanguard continued as our IGY satellite effort.

My interest and activity in upper-atmosphere research and instrumentation had led to my serving on Joseph Kaplan's "Long Playing Rocket" subcommittee of the Rocketry Panel in the National Academy's IGY committee; John Townsend, Milton Rosen and I had reported on the usefulness of a satellite in the IGY program without advocating any specific project. When the Technical Panel for the Earth Satellite Program was organized by the United States IGY committee in October 1955, I was a member, and proceeded to chair the Panel's Working Group on tracking and computation throughout the term of the IGY. Thus, though involved in JPL's support of a competing technical proposal, I was engaged in organizing operational support for the Vanguard mission. It was part of our task to encourage and coordinate various efforts, including international activities, in connection with observation and tracking stations for Vanguard.

The Microlock satellite communications, though now without a satellite mission, had further potential. The Jupiter missile project had to solve the problem of

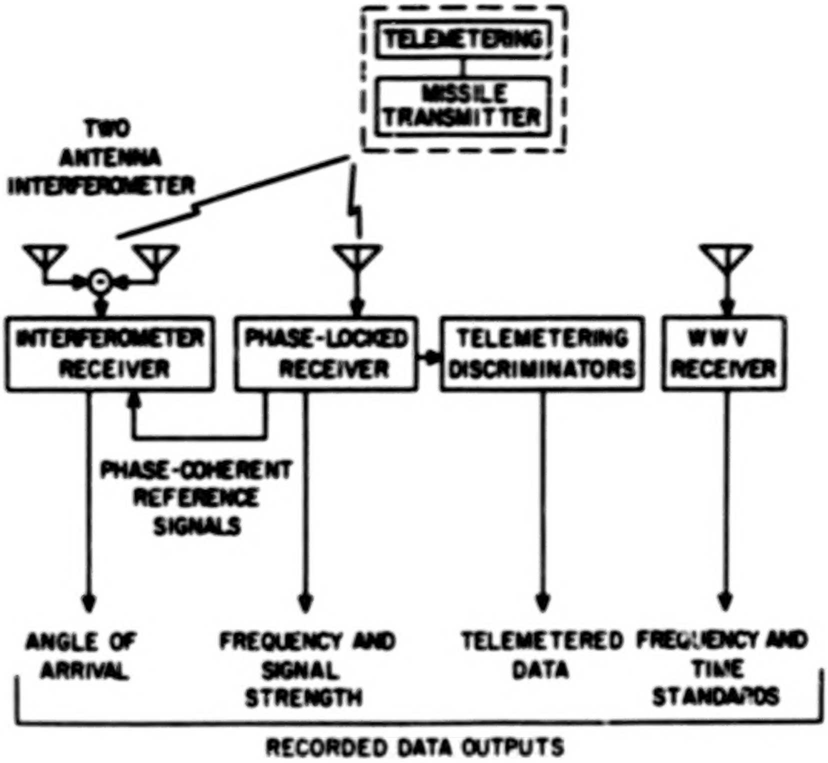
TELEMETERING
MISSILE TRANSMITTER
TWO ANTENNA INTERFEROMETER
INTERFEROMETER RECEIVER
PHASE-LOCKED RECEIVER
TELEMETERING DISCRIMINATORS
WWV RECEIVER
PHASE-COHERENT REFERENCE SIGNALS
ANGLE OF ARRIVAL
FREQUENCY AND SIGNAL STRENGTH
TELEMETERED DATA
FREQUENCY AND TIME STANDARDS
RECORDED DATA OUTPUTS

Fig. 19
Microlock System

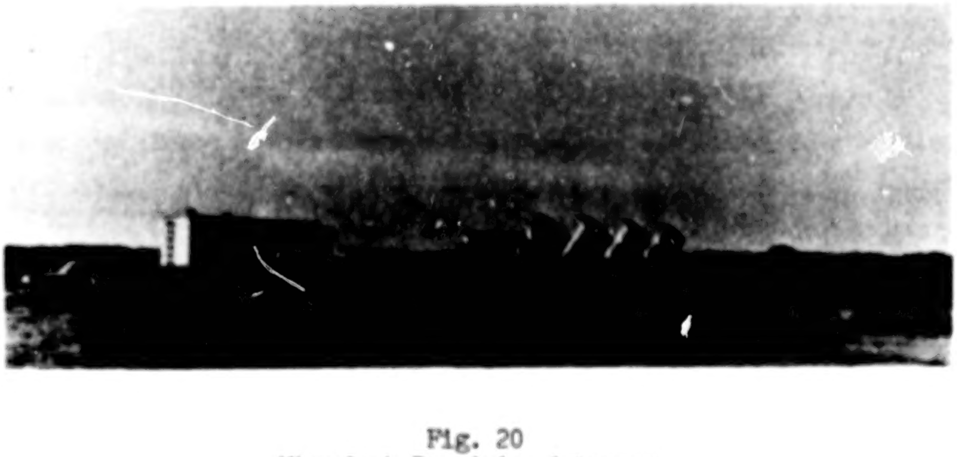

Fig. 20
Microlock Receiving Antennas

atmospheric re-entry at high velocity, and a heat dissipating nose cone of the ablation type was designed for this purpose. This design had to be tested under flight conditions—that is, actual reentry in a long-range ballistic trajectory—as soon as possible. The test cone would have to be instrumented so that its performance could be telemetered and its flight could be tracked for recovery of the probe. The April 1956 Microlock study report referred to this alternative possibility for the communications system. In fact, the test-vehicle program was already underway.

When Project Orbiter was terminated in late 1955, it was a fairly mature satellite system design based partly upon the use and adaptation of existing materials from missile projects. The long-range test mission required by Jupiter appeared to be very similar to the satellite launch, differing mainly in the lesser energy demand, and of course in the payload. The transformation of the Orbiter launch vehicle and telemetry into a Re-Entry Test Vehicle (RTV) would save time and money for the latter and provide at least a partial test demonstration of the former. It was a natural evolution.

First authorization of the RTV came in September 1955, after the Orbiter satellite proposal had been rejected. Flight design, laboratory and ground tests, and assembly of the system proceeded on schedule, and one year later the proof-test model was launched. The improved sub-scale Sergeant spinning clustered solid-rocket upper stages, and Microlock tracking and telemetry performed well, but only an inert payload was flown over the 3000-mile trajectory. The second test, the following May, carried a 1/10 scale Jupiter nose cone, but a guidance malfunction precluded its recovery. On the third test in August 1957, all systems performed excellently and the nose cone was recovered, validating its design for Jupiter and concluding the RTV program with several sets of flight hardware left over.

The successful orbiting of Sputnik 1 two months later created unprecedented excitement worldwide, and corresponding tension within the United States. Nowhere, I think, were feelings so strong as in the beleaguered Vanguard project, where the normal ups and downs of rocket and instrument development were now transformed by the press and public into gains and losses (mostly the latter) in a global competition. Among the former proponents of Project Orbiter, in Washington, Huntsville, and Pasadena, feelings of frustration also ran high in view of the recent RTV demonstration, and on occasion tempers wore thin. It was an enormous relief when the Army was authorized to proceed with its oft-proposed satellite development as a backup for Vanguard; I served as coordinator for this project with the Technical Panel for the Earth Satellite Program.

At the conclusion of the re-entry test program, General Medaris and Wernher von Braun had placed all the improved Redstone hardware in carefully controlled storage just in case they should be needed. Jack Froehlich, the JPL project manager, had reassigned all the one-fifth-scale Sergeant motors from the project to long-term life test, a device which had the same result. Enough Microlock ground sets had been constructed for extensive field testing as well as installation at the Cape for RTV tracking. But one critical element--the satellite itself--had to be created from scratch. Jack Froehlich, Homer Stewart, and I felt strongly that this task was logically the responsibility of the Laboratory, as it was so intimately involved with our communications system, our spectrum of skills, and our prior work in the Orbiter studies. The von Braun team, as a part of their strong motivation which had carried this cause unflaggingly for three years, and in view of their undeniable skill, felt that they should be responsible for the whole effort. The point was settled in favor of the Laboratory's participation, in a vigorous meeting before General Medaris,[29] and I think none of the team has since had cause to regret this resolution.

As coordinator for the Technical Panel for the Earth Satellite Program, I proposed that the Army satellite carry James Van Allen's cosmic-ray instrument (also flown in Vanguard), which I knew we could readily accommodate, and Maurice Dubin's micrometeorite sensors; four temperature measurements from inside and outside the payload package completed the list of telemetry sources. We expected the micrometeorite information and the temperature data to be of duel value, helping the immediate future design of satellite equipment as well as adding to scientific knowledge. The Van Allen experiment had been considered one of the high-priority IGY upper-atmosphere or space investigations, a judgement happily confirmed by its bountiful results in several missions.

We decided to fly two battery-powered radio transmitters, each carrying four standard channels of telemetry, dividing up the temperature sensor outputs and those of the two micrometeorite detectors, and carrying Van Allen's Geiger-tube measurements redundantly (Figure 21). Because of limited battery capacity, one transmitter operated at high power--fifty milliwatts--for a short time, and the other at only ten milliwatts for an expected two months. The high-power signal, which was amplitude-modulated, was intended to be recoverable by Project Vanguard's Minitrack ground stations and the like, but only our few Microlock sites were expected to be able to track the long-life, low-power signal.

When we were given the go-ahead, we had promised to be ready in 90 days, or by early February 1958; but scheduling at the Cape Canaveral test range allowed us only a few days at the end of January. This was indeed a hectic and busy time: not only had we to prepare the satellite, but the Sergeant missile and Jupiter radio-guidance programs were also coming to a head.

High winds forced postponement of the scheduled launch on January 29, 1958, and again the next day. But conditions improved on January 31 and the countdown carried to zero. I monitored the events from Washington, D.C., in the company with Wernher von Braun, James Van Allen, and many friends and colleagues of the IGY committee and the Department of Defense. Liftoff came at 10:48 p.m. EST (Figure 22). About five minutes later, Wernher turned to me and said, "It's yours now"--the first stage had been separated and the high-speed cluster ignited. From this point, of course, active guidance and Redstone telemetry

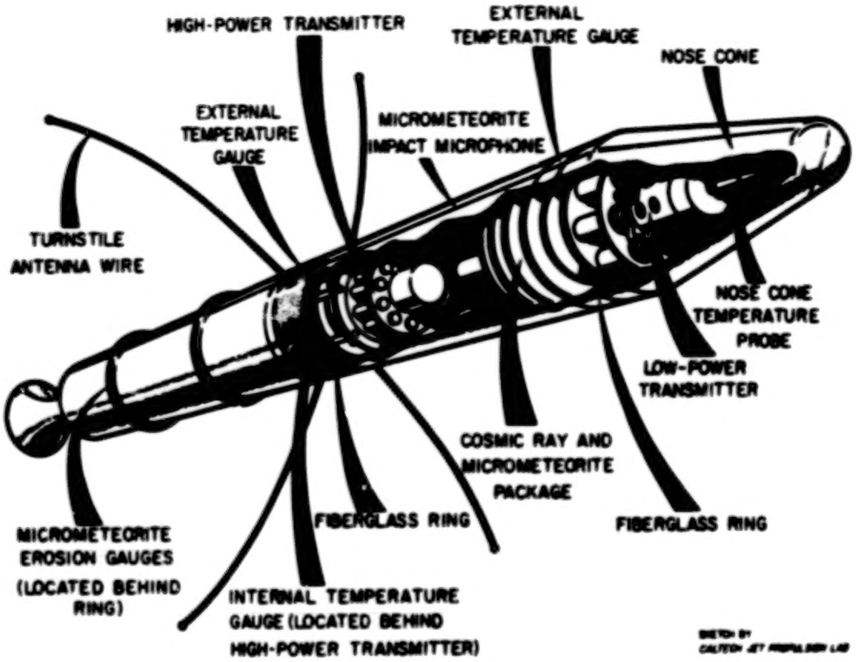
HIGH-POWER TRANSMITTER
EXTERNAL TEMPERATURE GAUGE
NOSE CONE
EXTERNAL TEMPERATURE GAUGE
MICROMETEORITE IMPACT MICROPHONE
TURNSTILE ANTENNA WIRE
NOSE CONE TEMPERATURE PROBE
LOW-POWER TRANSMITTER
COSMIC RAY AND MICROMETEORITE PACKAGE
FIBERGLASS RING
FIBERGLASS RING
MICROMETEORITE EROSION GAUGES (LOCATED BEHIND RING)
INTERNAL TEMPERATURE GAUGE (LOCATED BEHIND HIGH-POWER TRANSMITTER)
SKETCH BY CALTECH JET PROPULSION LAB

Fig. 21
Explorer 1 Satellite Instrumentation

were out out of the system, there was no command link any more, and after the antenna at Antigua Island lost the signal we would not hear from the space probe again until it passed over California, an hour and three-quarters afterward, if indeed it was in orbit. Several of us tried rough calculations of the flight path, unaware, as it turned out, that the still-strong jet stream above the Cape had boosted the bird to a high velocity and apogee, and hence a longer period. Finally, eight minutes late by our reckoning, the San Gabriel Valley Radio Club near Pasadena, and the Earthquake Valley Microlock site near San Diego, California, picked up the signal and called us. We were in orbit.

A few hours later at a press conference in the main hall of the National Academy of Sciences, I joined Wernher von Braun and James Van Allen, answering questions about the United States satellite. It was a turning point for each of us and the organizations we represented, perhaps most of all for the Laboratory and myself. The event was also symbolic of the mixing process between engineering and science, between the world and the research laboratory, which I have sought to trace through the dozen postwar years of growth of the Jet Propulsion Laboratory. Now the process had gone a step further, as I am sure Theodore von Kármán had expected, both through the Russian efforts and our own: it had mixed rocket technology with the universe, and reduced astronautics to practice at last.

Fig. 22
Explorer 1 Launch

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THE AERONOMY STORY: A MEMOIR[+]

Joseph Kaplan (USA)[++]

In the 20th century, recognition of the importance of the physics and chemistry of the Earth's upper atmosphere gave ionospheric research an increasingly strong theoretical base. Eventually, it paved the way for a broad spectrum of scientific activities that are now referred to as space research, or space science. No history of research in space can be complete, I submit, without an account of the part that aeronomy played in the origin of space science, and the role that it has continued to play in international space developments. In this brief memoir, I hope to look very briefly both at the past and the future of scientific research in space from the vantage of my personal experiences.

No attempt will be made here to present a complete picture of the way in which ionospheric research and aeronomy eventually led to research in space; rather, my account will be essentially autobiographical. Such an approach seems warranted because relatively few scientists early recognized the importance of the physics and chemistry of the Earth's upper atmosphere. Fewer still were responsible for the truly remarkable impact that upper atmospheric research had upon international cooperation in science in the 1950s. The creation of that remarkable enterprise, the International Geophysical Year (IGY), and the unprecedented continuation of space research thereafter, stems from the same taproot as aeronomy.

In a remarkable tribute published on the occasion of Sydney Chapman's eightieth birthday,[1] there are four short articles, each with the title "Ionospheric Physics and Aeronomy." The authors are J.A. Ratcliffe, David R. Bates, M. Nicolet and W.B. Hanson.[2] These four distinguished investigators call attention to the contributions that this great scientist made to two of the most exciting parts of what I now like to refer to as "Astrogeophysics." I refer, of course, to two important disciplines, ionospheric physics and aeronomy, experimental and theoretical methods for studying the Earth and its

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[+] Presented at the Fourth History Symposium of the International Academy of Astronautics, Constance, German Federal Republic, October 1970.

[++] Professor and Chairman, Department of Physics and Meteorology, UCLA, Chairman, U.S. National Committee for the IGY, National Academy of Sciences.

relations to the Sun. With this in mind, I could properly call this short memoir, "The Astrogeophysics Story," because more than any other single influence, the development of aeronomy under the inspiration of Sydney Chapman and a relatively small group of younger scientists has been responsible for the remarkable achievements and even more remarkable promise of science in space.

Chapman's interest in what is now called aeronomy began in the early 1920s, grew considerably in the 1930s, and continued unabated in the period following World War II. It was Chapman, with the help of a few of us who became increasingly active in aeronomy after the end of the war, who supported adoption of the new name, the International Association of Geomagnetism and Aeronomy (IAGA), for what was previously the International Association of Terrestrial Magnetism and Electricity. This change was announced at the 1954 General Assembly of the International Union of Geodesy and Geophysics (IUGG) in Rome, where Chapman was the presiding officer.

With this change in the name of one of the seven associations that made up the IUGG, aeronomy finally found an international home that it has occupied with increasing activity and distinction. I had the privilege of being president of the IAGA from 1957 to 1960, and of the IUGG from 1963-1967. I also had the pleasure of organizing a symposium on the upper atmosphere at the IUGG General Assembly held in Oslo, Norway, in 1958. It was there that aeronomy began to come of age. Subsequently, a regular series of upper atmosphere symposia have been held on the occasion of each successive IUGG General Assembly, and aeronomy has become an increasingly visible and important part of the IUGG and has begun to play a significant role in the activities of the International Union of Radio Science (URSI).

The story of aeronomy, as the reader may have guessed, is largely the story of Sydney Chapman's leadership of an ever-growing group of scientists. In addition to his outstanding scientific example, he brought these men together in the IAGA, helped them develop very important parts of astrogeophysics, and inspired them in many areas of international scientific cooperation that exist today in the International Council of Scientific Unions (ICSU). I say this because aeronomy was principally responsible for the interest in and eventual great success of the International Geophysical Year and its remarkable offspring, science in space.[3] Thus, it was no accident that Sydney Chapman served as the International President of the IGY. It was my honor to be Chairman of the U.S. National Committee for the IGY in the National Academy of Sciences. The story of the IGY and its continuing impact on international cooperation in many areas of science since 1957 is a dramatic example of the unforeseen effects of imaginative scientific efforts.

To provide future historians with otherwise inaccessible material and guidance on the unwritten story, I believe that scientists should write more autobiographical notes than have normally been customary. In this memoir, I should mention briefly my own scientific experiences that have played some part in the development of astrogeophysics. These will be almost entirely restricted to my role in the steadily increasing emphasis on laboratory studies of atmospheric and space physics, and to my related activities in the ICSU.

My interest in atmospheric physics really began in 1927 at Princeton University, with my discovery that the green line of atomic oxygen, a prominent feature to both the aurora and the airglow, could be observed in the spectrum of the Lewis-Rayleigh afterglow of nitrogen. This discovery came shortly after McLennan, also in 1927, had produced an oxygen green line in electrical discharges and identified it with the airglow line measured by Babcock and with the auroral green line. After reading J. C. McLennan's paper I realized that the line I had observed in the nitrogen afterglow was due to traces of oxygen that had been present. This accidental discovery led me to further studies deliberately directed at an understanding of the spectra of aurora and airglow.[4]

At the time of this first observation of the green auroral line of chemiluminescence, I knew practically nothing about either the aurora or the light of the night sky, as the airglow was then known. My principal interest involved the production and properties of atomic gases, based on my thesis on atomic hydrogen, written shortly after R. W. Wood had produced atomic hydrogen in electrical discharges. At Princeton I studied other atomic gases produced by electrical discharges, examining them outside the discharge. I started with further experiments on atomic hydrogen, but soon turned to nitrogen in an attempt to explain the long-lived afterglow of nitrogen, now known as the Lewis-Rayleigh afterglow.

The fact that the Lewis-Rayleigh afterglow was a property of nitrogen, combined with my accidental observation of the green line in the afterglow, led me to believe strongly that continued studies of the afterglow would help explain the then incompletely understood spectra of the aurora and the night airglow. My move in 1928 to the University of California, Los Angeles, brought me into close contact with Babcock and other Mt. Wilson astronomers, and their interest in the night airglow encouraged me to continue my studies of active nitrogen and its possible relationship to upper atmospheric spectra. Here, I should mention the 1928 Cario-Kaplan hypothesis which used both metastable atoms and molecules of nitrogen in an attempt to explain the long life of the afterglow of active nitrogen. None of the metastable atoms or the metastable molecule of nitrogen which were used by Cario and myself to explain the afterglow had ever been directly observed in the laboratory; the green line of oxygen was a "forbidden line" that originated on a low-lying metastable state of atomic oxygen. The idea of introducing metastable states into the nitrogen afterglow problem really came from the discovery of the green line excitation referred to earlier.

During the 1930s I continued studies of active nitrogen that led to the discoveries of other nitrogen afterglows, and to the direct identification of metastable nitrogen molecules and atoms in both afterglows and discharges. These studies also led me to the identification of forbidden nitrogen atomic and molecular radiations in auroral spectra. My relatively simple laboratory studies helped considerably to clarify our understanding of the Earth's upper atmosphere and at the same time added to our knowledge of the atoms and molecules of nitrogen and oxygen. In my own mind at least, I am certain these experiments helped me to realize the importance of the upper atmosphere as a great and complex laboratory in which the chemistry and physics of oxygen, nitrogen and hydrogen could be studied.

The introduction of rockets for scientific purposes in the 1940s led to exciting and rapid developments in aeronomy.[5] Nearly all that we had learned before instrumented rockets became available was subject to changes that this new tool made possible. We could now look at the Sun without the interference of the Earth's atmosphere, and we could study the high atmosphere directly.

My role in these post-war developments, in the second half of the forties and the early fifties, helped stimulate interest in the upper atmosphere. However, I also trained a number of outstanding students, some of whom have distinguished themselves in atmospheric and space research. In addition, I found myself in a strong position to develop support for the then relatively expensive rocket experiments, and in this way to move into the great period of rocket and space studies that occurred during the IGY.[6] I have no doubts regarding the quality and quantity of exciting scientific results that will come to mankind as a result of the initiation of the space program during the IGY.

Part of the citation for the Hodgkins Medal and Prize of the Smithsonian Institution, which I shared in 1965 with S. Chapman and M. Nicolet, stated that the award was given for continuing effort in making and inspiring laboratory experiments directed at understanding the observed radiations from the upper atmosphere, anticipating the significance of space research, major contributions to the International Geophysical Year, and enthusiastic support of international geophysical research. I mention this only because it is very unlikely that the award would have come to me had I not accidentally produced the green auroral line in chemiluminescence many years before.

I submit strongly that as important as the scientific parts of the aeronomy story may be, the social, political and economic aspects of aeronomy, ionospheric physics, solar-terrestrial relationships, and science in space may be far more significant historically. Scientists must learn to tell the story their discoveries hold for society if support for making accidental discoveries in fundamental experimentation is to continue. My part in the aeronomy story is but one of many examples of the significance of such unforeseen discoveries.

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THE VIKING ROCKET: A MEMOIR[+]

Milton W. Rosen (USA)[++]

Quite often, history is a matter of recollection, based upon unorganized documents, memoranda, and recalled conversations--some more or less relevant. In the present case, that of the Viking rocket, we are fortunate in having two sources written at the time the events were occurring. The first is the complete set of Naval Research Laboratory (NRL) Rocket Research reports[+++] that describe in technical and chronological detail the histories of each of the ten Viking rockets--their design, instrumentation, the course of field operations, and the subsequent analysis of results. The second source is my book The Viking Rocket Story, that covers the same ground, but in less technical and more readable prose. Although it speaks about rockets, the book deals primarily with people--the men who built, tested, and launched the rockets, whose worries, fears and frustrations are portrayed along with their joys and triumphs.

In most cases I will let the sources speak for themselves, quoting directly where possible, but interpolating some present thoughts when desirable in the interest of clarity. The story of Viking is best told through its launchings, and I have chosen four of the twelve total. These four include the first one, the beginning, so to speak; the fourth, a launching at sea in the mid-Pacific; the eighth, sometimes referred to as the high-altitude static; and the tenth or last of the original series. Summary performance data on all twelve Vikings appears in Tables I and II at the end of this paper.

The launchings took place (except for the Pacific Ocean) at the White Sands Proving Ground, an Army testing installation in a remote, desert location in Southern New Mexico. The Proving Ground occupies a natural basin (the Tularosa Basin) ringed by mountains; the basin is about 50 miles across and 100 miles long, roughly the size of the state of Vermont.

[+] Presented at the Sixth History of Astronautics Symposium, International Academy of Astronautics, Vienna, Austria, October 1972.

[++] Secretary, Space Science Board of the National Academy of Sciences, Washington, D. C.

[+++] Principal authors: Milton W. Rosen, James M. Bridger, and Alton E. Jones.

But now to the story, quoting first from the Naval Research Laboratory report on the first Viking flight test.

> On May 3, 1949, the first Viking rocket launching and flight was achieved at White Sands Proving Ground, New Mexico, where the rocket reached a peak altitude of 50 miles. The Viking, formerly known under the name Neptune, was originally conceived by NRL scientists as a vehicle for carrying research instruments into the upper atmosphere. For several years, the V-2 rocket has been used for this purpose with considerable success. But the supply of V-2's is limited and as a vehicle for research the V-2 has certain disadvantages with regard to payload flexibility and stability in flight. In sponsoring Viking's development the Laboratory's purpose is two-fold: to obtain an improved vehicle for the continuance of upper-air research and to advance the rocket art in the United States. The development program encompasses the design, fabrication and launching of ten Vikings of which each rocket is an improvement over its predecessor. The field operations include one or more static firings of each assembled rocket, after which the flight firing is conducted. The program is so devised that the experience gained from each rocket contributes to the design of the next and all other succeeding rockets of the series.
>
> Description of the rocket. The Viking is a high altitude sounding rocket designed to carry a 500 pound instrument payload to altitudes of about 100 miles in nearly vertical flight. The vehicle is a conical tipped cylinder, 45 feet in length and 32 inches in diameter. Four swept-back fins (total span, 9 feet and 2 1/2 inches) are spaced equidistantly around the aft end of the cylinder. The weight at takeoff is about 10,000 pounds including 7,000 pounds of propellants.
>
> Propulsive force is obtained from a 20,000 pound (sea level) thrust rocket engine. The rocket propellants, liquid oxygen and ethyl alcohol, are fed to the engine by means of a turbopump. The turbine is driven by steam obtained from the decomposition of hydrogen peroxide. Nitrogen gas is used for pressurizing the pump and rocket propellant tanks, for actuating pneumatic controls, and for coasting-flight stabilization. During powered flight the vehicle is stabilized in pitch, yaw and roll by means of internal controls. Error signals from gyro pick-offs are differentiated, amplified, and fed to solenoid-controlled hydraulic valves. For pitch and yaw corrections, these valves control hydraulic servos which rotate the gimbal-mounted thrust unit about either or both of two axes. Roll correction is obtained by diverting turbine exhaust steam through ports on two opposite fins and by means of trim tabs. The roll hydraulic valves control the position of steam deflectors which are mechanically linked to trim tabs on the same two fins.
>
> After burnings' end the error sensing system is switched to control an array of gas jets. These jets provide correcting moments in pitch, yaw, and roll to remove error moments introduced at or after burning's end. This post-cutoff stabilization system operates for about the first 20 seconds of coasting flight. (Later versions of Viking incorporated an active coasting flight control system, using steam jets that operated during all of coasting flight to stabilize the vehicle in pitch, yaw and roll. The steam was obtained from decomposition of hydrogen peroxide.)
>
> The rocket is serviced with ethyl alcohol, hydrogen peroxide, and liquid oxygen, in the order mentioned. Then the gas storage sphere is charged with nitrogen to a pressure of 3400 psig. The final operations are conducted from two remote panels installed in the blockhouse at White Sands Proving Ground. The gyros are nulled and the thrust unit is centered from the controls panel. The propellant tanks are pressurized and the rocket is fired from the firing desk.

> In anticipation of the extensive field operations required for launchings of ten Vikings, a rocket servicing crew was formed at the Martin Company (the rocket's builder) in May 1948. The crew was composed of engineers and technicians, eight from the Martin Company, three from the Naval Research Laboratory and one from Reaction Motors (the engine builder). [Note that the launching crew consisted of only twelve people, assisted later by some Naval personnel at White Sands.]
>
> The rocket arrived in New Mexico on January 15, 1949, and was transported to the Navy technical area. A receiving inspection was made, components were bench tested, installations were made and functional tests of the control system and power plant were conducted for the remainder of the month. After completion of a hangar pre-flight test, the rocket was ready for transportation to the launching area.
>
> On February 28, the rocket was erected in the launching area in preparation for a static firing. The static firing of a Viking is an operational test in which the completely assembled rocket is fired and the performance of the various systems and components are measured using the flight telemetering system. The rocket is secured to the launching stand by bolts which engage the support pads on the fins. Temporary piping attached to the launch stand is used to spray carbon dioxide and water into the tail section in case of fire. The normal duration of a static firing is 30 seconds, about which time the rocket should cut itself off through operation of an alcohol low-level sensor. However, an emergency shut-down can be made from the firing panel in the blockhouse.

I will not continue the harrowing story of the long series of attempts to static fire and flight test the first Viking. Suffice it to say that this operation continued over a span of two months during which various parts of the rocket either failed or refused to operate properly. The most notorious of these bad performers were the valves used to vent oxygen from the rocket propellant tank. These valves had the annoying property of freezing in the open position so that they could not be closed in preparation for a firing. This malfunction occurred repeatedly, despite replacement of the valves and redesign in the field. We pick up the story on May 3 after the static firing and in preparation for the flight, with the vent valves behaving in characteristic fashion.

> The first attempt on this date was halted at 8:30 in the morning when the vent valves failed to close properly. Because the weather was becoming progressively worse and because there was a possibility that the rocket would have to be returned to the hangar if the launching were not conducted on this date, the ensuing operations had to be conducted with rapidity. There was no opportunity to top off oxygen although it was known that the oxygen level would be reduced by boil-off.
>
> A series of test operations were quickly conducted in an effort to find a procedure for ensuring closing of the oxygen vent valves. The procedure finally used consisted of repeatedly closing and opening the vents prior to pressurization. It was, of course, not possible to keep the vents permanently closed because of the pressure rise due to oxygen boil-off. The previously described expedient finally resulted in a successful closing of the oxygen vents. The rocket was pressurized and launched successfully at 0914 Mountain Standard Time on May 3, 1948.

The launching is more graphically related in The Viking Rocket Story. The count-down passed from X-minus-20-seconds to minus 3 when the command was given to fire.

> The plug dropped, steam shot out of the turbine exhaust, the whole rocket shuddered, and then slowly, majestically, rose up from the launching stand as its long, fiery jet played upon the ground.
>
> Takeoff!
>
> We could no longer see it from the blockhouse, but we could hear it roaring overhead--and each of us prayed for it to keep burning. Sixty-five seconds, the most we could hope for with our oxygen low, might get us close to a hundred miles. The seconds ticked away slowly and then suddenly, much too early--burning's end. What was it--fifty-three, fifty-four, perhaps fifty-five seconds? The time was very uncertain. The vital numbers started coming over the phone from C Station. Altitude at burning's end - 17 miles, velocity - 3,300 feet per second, time to peak - 160 seconds, peak altitude - 51 miles. I was disappointed, and so were the others, at first. We felt better when we heard the remarks from C Station. Good flight! . . . Straight up the middle of the range! Fifty-one miles - damn good for a first rocket!
>
> We filed out of the blockhouse, all of us except Pres Layton, who was slumped over the firing desk, exhausted. We walked out to the launching stand and stood around it. It was still warm. One of the crew looked up and said, "I can't believe it's gone. I can still see it standing there."

We continue this Viking history with an account of the shipboard launching as related in the NRL Rocket Research report.

> On May 11, 1950, Viking Rocket No. 4 was launched from the USS NORTON SOUND at the equator in the Pacific Ocean. This rocket reached an altitude of 105 miles with a 900-pound payload of instruments designed to study cosmic radiation and atmospheric pressure. Early in the program, NRL was requested to design the Viking to test methods of launching large, stabilized rockets from shipboard. In August 1949, the laboratory requested use of the NORTON SOUND to fire a Viking at sea on or about May 10, 1950. The Chief of Naval Operations approved this request and initiated "Project Reach" for the launching of an upper-atmosphere research vehicle for the study of cosmic radiation at the equator, and gaining practical experience in the shipboard handling, launching and tracking of large bombardment-type missiles. The ensuing preparations, which occupied a period of seven months, culminated in the first successful firing and flight at sea of a large stabilized rocket.
>
> The first three Vikings were fired at the White Sands Proving Ground in New Mexico. Although the highest altitude was 50 miles, the performance of each of these models was sufficiently good to demonstrate the soundness to the rocket's basic design. Static firings of each fully-assembled rocket were conducted at White Sands prior to the flights. Full use of the vehicle for carrying upper-atmosphere instruments was initiated in Viking 3 which was devoted to cosmic-ray research, solar spectroscopy, and atmospheric pressure measurements.
>
> Early in the Viking program, both scientific and military interest were evinced in the firing of a large rocket from shipboard. The feasibility of such a shipboard launching was demonstrated when a V-2 was fired from the USS MIDWAY in September 1947. The principal scientific value of shipboard launchings is to extend widely the areas over which atmospheric studies can be made. From 1946 until 1949 the rocket upper-air research program in the United States was confined to the White Sands, New Mexico area. Although a great deal more remains to be done at White Sands, it is essential that studies be made at numerous geographical locations. For example, in the case of cosmic radiation,

> geomagnetic latitude is important, since the ability of the earth's magnetic field to deflect an incoming particle depends in part upon the magnetic field direction, which varies with geomagnetic latitude. In essence, the earth's magnetic field performs a geographical sorting, in accordance with cosmic ray energies.
>
> The military value of firing a large rocket at sea is immediately apparent. Because of the ship's mobility, the launching area is extended to include all accessible waters, and thus to the range of the missile there is added the range of the ship. [Bear in mind that this was written about ten years before the first launching of a Polaris missile from a naval vessel.]
>
> The Navy Department in 1947, designated the USS NORTON SOUND as a facility for the evaluation of guided missiles and as a mobile launching site for scientific investigations requiring specific geographic locations. Previously, a seaplane tender, the NORTON SOUND was converted to an experimental guided-missile ship in the summer of 1948. Also in 1948, NRL issued a report that laid the plans for a shipboard launching. The report dealt with handling and launching problems, telemetry, tracking and recovery of rocket-borne equipment, and also pointed out the necessity of several land-based firings before attempting a Viking launching at sea. The performance of Viking No. 1, however, was sufficiently good to enable the laboratory to propose the 4th Viking as the vehicle for the shipboard launching.

The significant difference in a shipboard firing arose from the motions of the ship, its pitch and roll, that could be a problem in the separation of the rocket vehicle at launching. The design of the launcher was critical, hence, we chose a simple, fixed mechanism: a pair of rails aligned vertically at one side of the rocket. Two sets of rollers, one set at the vehicle's aft end, the other set 10 feet forward of the aft end, engaged the rails and constrained the rocket to move with the ship until its ascent had freed it from the railway.

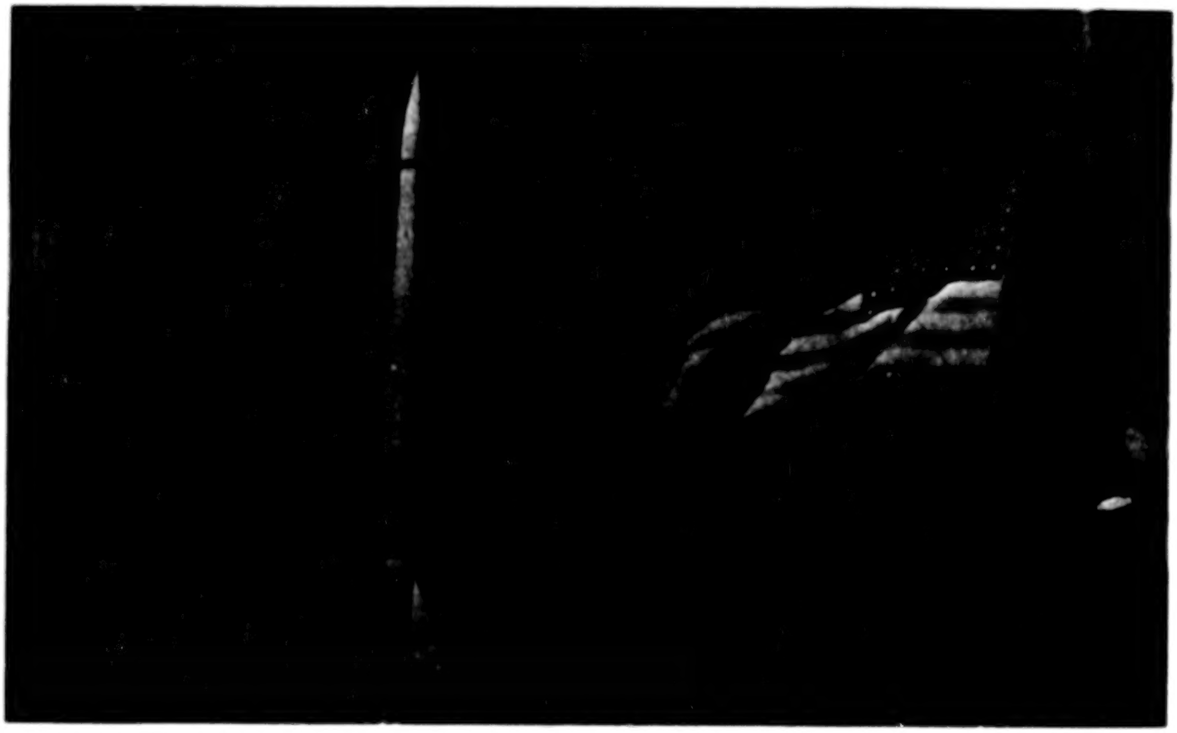
Two dummy Viking missiles were built in order to test the launching technique prior to the firing of Viking 4. Each dummy duplicated the live rocket in the essential features required for the launch test. Externally, it was nearly identical, using the regular tail and tank section skins. Internally, duplication of the actual rocket ceased. The dummy missile was powered by a solid propellant JATO which produced an average thrust of 5700 pounds for 3.5 seconds. The JATO bottle was cocked at an angle from the missile's axis in order to carry it away from the ship--about 400 feet at impact. The gross weight of the dummy was made proportional to the thrust, so as to produce the same acceleration and velocity in the tower as those of the actual Viking.

The first dummy missile was fired in the Santa Barbara Channel off the coast of California two weeks before the NORTON SOUND sailed on the expedition to the mid-Pacific. The firing had been delayed several days in the search for sea conditions producing ship's motions of sufficient amplitude. The idea was to fire the dummy missile at the worst possible moment in the ship's roll cycle so as to produce the most severe "tip-off." Then the live Viking would be fired at the most favorable time in the ship's motion and would be assured a safe separation. The dummy was fired successfully and confirmed the calculations of "tip-off" due to ship's motions.

The NORTON SOUND sailed from Port Hueneme on April 26, 1950, and was joined by the destroyer OSBOURN 250 miles west of San Diego on the following morning. This fleet of two vessels then proceeded, over a period of 10 days, to the launching area at the geomagnetic equator in the vicinity of Christmas Island, some 2000 miles due south of Hawaii.

The NRL report describes a long series of difficulties and mishaps associated with operations at sea. The high humidity of the tropical night air was a source of great concern, depositing moisture on lines and connectors and shorting electrical circuits. We had to learn how to operate the rocket's vibration-sensitive control system on a continually moving ship. Nevertheless, after several trials Viking 4 was launched successfully from the deck of the NORTON SOUND as it proceeded along the equator near Christmas Island (Figure 1). The dispatch from the ship's Captain to the Navy Department read:

> SEA-GOING VIKING SERIAL NUMBER 4 WITH HEAVY INSTRUMENT LOAD LAUNCHED ON GEOMAGNETIC EQUATOR 1600 LOCAL TIME ELEVENTH MAY X ALL SCIENTIFIC OBJECTIVES



Official U.S. Navy Photograph

Fig. 1

> ACHIEVED MUCH VALUABLE EXPERIENCE GAINED NO CASUALTIES X MAXIMUM ALTITUDE 106.4 STATUTE MILES MORE THAN DOUBLE PREVIOUS VIKING RECORDS AND NEW RECORD FOR SHIP-LAUNCHED MISSILE X MANEUVERED BENEATH 3/10 CLOUD COVER TO EXCELLENT OPENING FOR FLIGHT X OSBOURN AND HELICOPTER SUPPORT OPERATIONS HIGHLY SUCCESSFUL X PROCEEDING PEARL X

On the return trip to Hawaii some 250 members of the Viking project complement and the ship's crew, who had never before crossed the equator, were initiated into the mysteries of the Society of Shell-backs.

We resume the Viking history with the NRL report on Viking 8.

> In the Fall of 1950 NRL and the Martin Company began considering major revisions to the Viking rocket in order to extend its altitude ceiling. The original design requirements (500 pounds of payload to 100 miles) had already been exceeded by Viking 4, and minor changes in Vikings 5 and 6 promised to raise the altitude ceiling to 125 miles. Two avenues lay open for further increase: (1) improvement in specific impulse, and (2) improvement in mass ratio. Attempts to increase the engine specific impulse without major redisign (principally, by injector redesign) had not been successful, although such improvement is certainly possible. Mass ratio had been improved continually through the first seven Vikings. A large gain could be made only by a significant increase in propellant capacity without a corresponding increase in structural weight. This was the primary purpose of the redesign. A secondary aim was the rearrangement of power plant and control equipment to improve accessibility of the various components.
>
> The design changes, originally scheduled for Viking 7, were achieved too late for incorporation in that rocket, which was built to the original configuration. It achieved a record altitude of 136 miles. Viking 8, then was the first model of the new design. The rocket was never brought to the flight firing stage. During the static firing at White Sands Proving Ground, coupled vibrations between the motor and structure exceeded static load limits and caused the rocket to tear loose from the launching stand after 15 seconds of burning. Flying in a static firing condition the rocket continued under power until it was cut off by radio at 61 seconds. It reached an altitude of 20,000 feet and landed four miles from the launching site.

A more graphic description of the events surrounding this firing is taken from The Viking Rocket Story:

> As the second hand on the large clock swept through the last minute before X-minus-15, we became quiet. Jim Hartman was at the firing desk, substituting for Hardin who took over the fire-watcher station and was peering at the rocket through binoculars. Mason was at the controls panel; Munnell and I were between the two operators a step behind them. At X-minus-15-minutes Hartman turned on master power and Mason switched the gyros to erect. When after two minutes the gyro meters did not come to zero, Mason requested permission to erect the gyros from the panel controls. Munnell assented. This procedure left some uncertainty about the true position of the gyros, a condition that would never be tolerated for a flight, but could be accepted on static. The count proceeded without a hold. At X-minus-1-minute the rocket was pressurized. Munnell, Hardin, and I hung over Jim Hartman at the firing desk as the meter needles swung up, wavered and then stood still.
>
> Forty-five seconds . . . Hardin nodded to Munnell, indicating that he was satisfied with the pressure levels. Ed flicked on his green light. The rocket was cleared for firing.

Thirty-five seconds . . . I turned to the telemeter men and called for recorders. The work was relayed to the telemeter house seven miles uprange. We waited.

Twenty-five seconds . . . The answer came back. Recorders were on. Lieutenant Cooper, the Navy project officer, started the count down in a hushed voice.

Twenty, nineteen, eighteen, seventeen . . .

Twelve, eleven, ten, nine . . . the ready lights were all green.

Five, four, three, two . . . Fire!

Igniter on . . . plug drop . . . fire light . . . the motor had started. At plus four seconds, the firing panel meters wavered and dropped to lower levels. I looked at Ed to see if he had noticed it. He had and was glancing alternately at the panel meters and the rocket. At nine seconds we heard a dull throbbing sound in the jet blast. The thought flashed through my mind: Should we cut it or let it run longer to get enough time on the record? How long was enough? At thirteen seconds the rocket started heaving almost imperceptibly. Then without warning, before our horrified eyes, it tore loose from the stand and started slowly to rise. We saw the tubes and cables that were connected to the rocket for static test snap one by one as the tail moved upward out of our sight. A great cloud of dust billowed toward us as the mighty roar of a flying rocket bore down upon the blockhouse roof.

Takeoff! The rocket has broken loose! The rocket is flying! The alarming words flashed out from the blockhouse. At the telemeter station the men dashed up to the roof to start training their antennas which had been lashed down for the static.

Plus twenty-five seconds . . . Nat Wagner yelled at me across the blockhouse, "Shall we cut it?"

As the seconds slipped away I pondered this deadly question. The rocket was lost, with only half a tank of alcohol it could not go far, not possibly leave the range. If we cut it too early, it might fall on one of the installations close to the launching area.

"No!" I shouted back. "Don't cut it. You don't know where the rocket is!"

Thirty-five seconds . . . Nat and I were staring at each other across the blockhouse. I knew he wanted to stop the rocket and I felt it would be a mistake. There was no time to explain the conditions - a tank half full to start, fifteen seconds of fuel burned on the ground.

Fifty-five seconds . . . "We're going to cut it," Nat announced. He pressed the button. We waited in silence, not knowing whether he had stopped the rocket or whether it had cut itself off previously. We had ceased to hear its sound many seconds before.

Finally, at 120 seconds, we heard a muffled explosion. We looked at each other; that must be impact. We rushed out of the blockhouse and scanned the horizon. To the southeast we saw a gray mushroom of smoke rising slowly against the pale, blue sky. We estimated its distance as four or five miles. I walked toward the launching stand. The two tie-down blocks from the rocket were still in place, bolted to the crossbeams of the stand. Attached to the blocks were a few scraps of structure that had been torn from the rocket when it broke loose. The two shiny stubs of aluminum pointed upward, silent evidence of the rocket's awful power.

Returning to the NRL rocket research report we quote as follows:

> The destruction of Viking 8 can be attributed to a combination of factors. Of primary importance was the failure to shut off the power plant before the rocket had broken loose. Although cutoff could have been effected at any time prior to 14 seconds of burning, no action was taken to initiate it. If, in the light of foregoing paragraphs, it appears incredible that no action was taken, it should be remembered that 4 to 6 months are required to assemble and analyze the data presented in this report, whereas the firing crew had 5 to 8 seconds in which to recognize the situation, to evaluate it, and to take appropriate action. The mistake in this case arose from the fact that judgment was required in order to initiate action. Under the emergency procedures used in early Viking firings, cutoff would have been given at about 5 seconds when gages on the firing panel indicated operation at reduced thrust. But during the course of 10 static firings without mishap, there was a tendency to relax the rigid emergency procedures and to rely more on the operator's judgment, since the characteristics of the rocket had become better known and the crew more experienced. On Viking 5, a low thrust static firing was allowed to run for 30 seconds. It should have been realized that, because of the redesign, Viking 8 required the same degree of caution as Viking 1.
>
> The decision to restrain the airframe during static firing at two points instead of four was reached early in the design period many months before the firing. Subsequent structural analyses and static load tests showed this scheme to be adequate for normal conditions, plus specified factors of safety. That the design was inadequate for the abnormal conditions of Viking 8 is now obvious. The question of how far to go in protecting a rocket against abnormal conditions has never been answered satisfactorily. Once a specific condition has occurred, corrective action is always taken. But, should an attempt be made to foresee and design for as many abnormal conditions as possible? Increased factors of safety are obtained only at the expense of lower performance. One lesson can be learned from the Viking 8 experience— for a vehicle that is still in an experimental stage of development, adequacy alone is not a sufficient criterion.

This history closes with an account of the launching of Viking 10 introduced as follows in the NRL report:

> The 10th Viking, launched from the Naval site at the White Sands Proving Grounds, at 10:00 a.m. on May 7, 1954, carried 675 pounds of instruments to a peak altitude of 136 miles. The first attempt to fire this rocket on June 6, 1953, resulted in an explosion and fire which destroyed the combustion cylinder and severely damaged several other power plants and control components. The rocket was returned to the contractor's plant for repair and an extensive investigation of the nature and causes of the explosion was made.

A graphic description of this first firing attempt is taken from The Viking Rocket Story:

> We were in the blockhouse again, shortly after noon and went through a second count down, punctuated by brief holds, the last of which occurred at X-minus-2-minutes.
>
> "On-the-mark time will be running at X-minus-2-minutes.
>
> . . . Mark! . . . X-minus-2-minutes."

Hardin put the safety key in the firing desk; until this key was engaged the rocket could not be pressurized or fired.

"Coming up on X-minus-1-minute . . . Mark! . . . X-minus-1-minute . . . fifty seconds . . . forty-five seconds . . . forty seconds . . . thirty-five seconds . . . Recorder on! . . . twenty-five seconds . . . Recorders are on! . . . Twenty, nineteen, eighteen, seventeen . . .

At ten, Pitts' voice was joined by the others who were relaying the time in unison so that the rhythmic count took on the air of a religious chant.

"Seven, six, five, four, three, two, one, zero!"

Hardin pushed the firing switch, the plug dropped from the rocket's nose, and the turbine exhaust belched forth a stream of ominous black smoke. Suddenly, without warning, in less than a second, a terrific blast smote the blockhouse as the rocket's tail seemed to disintegrate. White steam, orange flames and gleaming metal shot out from the tail in an explosion of unbelievable violence. Then a great ball of flame rose from the tail and enveloped the rocket.

"Misfire! The rocket's on fire! No shoot, the rocket is burning."

When we saw the explosion, Hardin, without hesitation, threw the firing switch to cutoff. Schlecter ordered the carbon dioxide and all water sprays turned on. Then he turned to Pitts and said, "The rocket is out of our control. It belongs to the firemen now."

The four giant fog nozzles at the edge of the launching mat, each one pointed at the base of the rocket, were turned on. The water came out with agonizing slowness, trickling toward the flaming rocket. When, finally, the full force of the spray hit the tail, a great cloud of steam obliterated the area.

Joe Pitts in the center of the blockhouse was being bombarded with questions and suggestions. His primary concern, and rightly so, was the safety of all personnel in the immediate area. Those in the blockhouse could be considered safe, as long as they stayed inside, but there were dozens of people outside, several hundred yards from the rocket, some of whom, out of curiosity, might approach it.

"All personnel stand clear, stand clear, take cover! The rocket is burning." Pitts' words blared forth from the loudspeakers outside the blockhouse. "All personnel stand clear and take cover!"

I surveyed the situation and was sick at heart. Schlecter and Pitts had done all that could be done; there was nothing left now but waiting. The rocket was still standing, as if by a miracle, but it would be suicide to approach it. Captain Quirk, the Navy commanding officer, was in the blockhouse and had set a guard on the door - no one could leave. I saw the firing pit fill up with white vapor. Oxygen, I surmised - we must have dumped the whole tankful into the pit. But there were still three tons of inflammable alcohol in the upper tank and, worse yet, four hundred pounds of highly explosive peroxide in the tail with a fire, now confined to the east quadrant, only a few inches away. Surely the peroxide would go up any minute and then all would be over. I looked at my watch - it was twelve-thirty.

> Twelve-thirty-two. The blockhouse was a beehive of excitement; knots of men packed at each of the three windows, watching the rocket, a scant two hundred feet away, with a horrified fascination. The other men were scattered about the blockhouse interior, some in small groups, others milling around and glancing furtively from time to time at the small open patches of window space. Joe Pitts was at his rostrum in the center of the room, and was busy receiving reports and relaying information to people all over the range. Now and then, individual voices could be heard above the incessant buzz of conversation.
>
> "Stand by, no shoot, no shoot. The missile is burning."

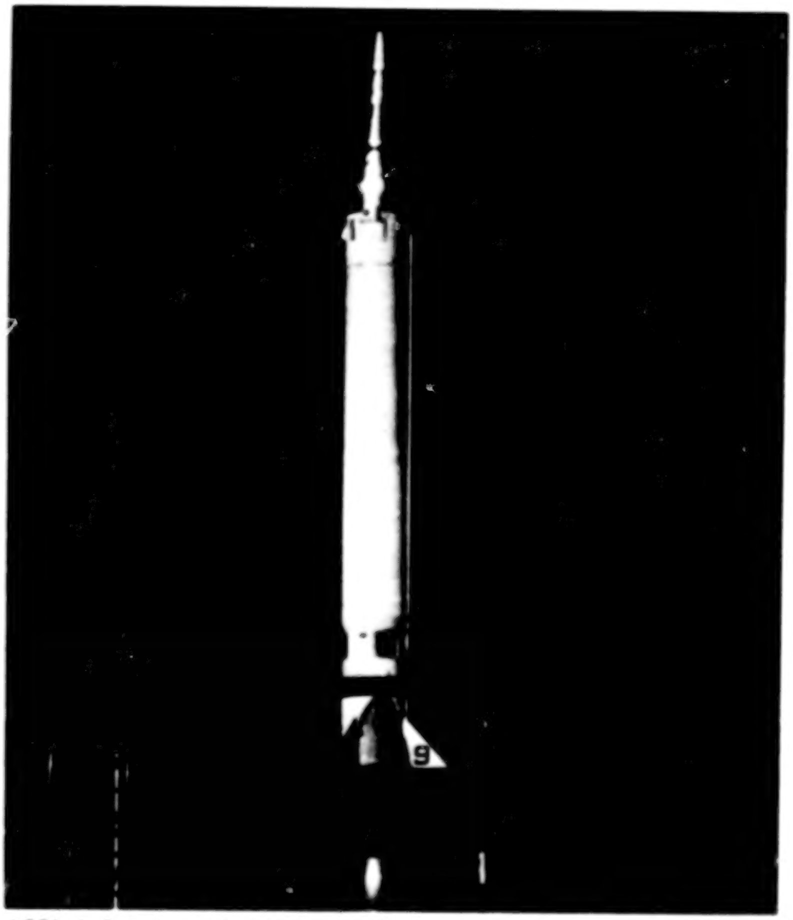
Five hours later the scene is described as follows:

> Five-thirty. Although the fog nozzles and fire hoses had been turned off two hours before, the tail was still dripping water into the pit. The metal was cold, but everywhere there were signs of the searing heat that had followed the explosion. All the tail access doors, both large and small, had been ripped off and hurled from the rocket, some as far as a hundred feet. Large peices of skin material were missing, and from the fused edges of the jagged holes we could tell that some of the metal had been vaporized. Inside the blackened shell there was only devastation. The east quadrant was gutted, the controls can had exploded, and inside there was a fused mass of metal, wire and glass. The fire wall above the motor was dished upward, with a large gaping hole at its center. The motor itself was a most horrible sight. It had been blown apart into two pieces; its massive head was jammed against the gimbal ring, and its nozzle hung awkwardly by two alcohol hoses like a broken jaw.
>
> Five-fifty-eight. The sun had dropped behind the Organs and the Western sky was red as we climbed into our vehicles and slowly drove away from the launching area. We had been there fourteen hours, a long, unforgettable day that had seen us push a button that brought instant disaster and, finally, had watched a valiant fight to save a crippled machine that meant so much to all of us. As I looked back at the white rocket, now sheltered by its gantry framework, I could hear the oft-repeated words, "You'll never see this rocket fly."
>
> I resolved that somehow, someday, Viking 10 would fly.

The rocket was rebuilt, returned to the White Sands Proving Ground and successfully launched almost a year later to peak altitude of 136 miles (Figure 2). It made valuable measurements of upper atmospheric pressure, density, and winds, and determined the ion content of the atmosphere above an altitude of 100 kilometers.

From the foregoing stories of Viking firings, there emerge some of its basic contributions to the progress of rocket design and development. Viking was the first large rocket to successfully employ the gimballed motor that became a standard feature of Thor, Atlas, Titan, and Saturn, and almost every rocket of the last twenty years. We suffered with and mastered the problems of instability and vibration in gimballed motor systems and adopted the transfer function method of analysis.

Through Viking innovations, the mass ratio (MR) of rocket vehicles was advanced to much higher levels. Viking spanned the gap between the V-2's MR of 0.67 and the Thor-Atlas values of 0.94. Integral tanks, extensive use of aluminum and magnesium, low thrust-to-weight ratio—these were some of the measures that ploughed the ground leading to high mass ratio.



Official U.S. Navy Photograph

Fig. 2

Because of its extensive use for upper air research, Viking had to develop a coasting-flight stabilization system consisting of a gyro-controlled array of gas jets. Such a system has been a standard feature of succeeding launch vehicles and spacecraft, particularly those intended for manned flight.

An unusual feature of the Viking experience is the small amount of funds and manpower that were required. The entire Viking development and launching program, extending over a period of eight years, cost no more than five million dollars, and absorbed the energies of no more than 100 engineers, technicians, and factory workers. Economy was a necessity--and we made of necessity a virtue. Will such times ever return to our field?

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TABLE I. PERFORMANCE DATA (VIKINGS 1 THROUGH 12)

Viking No.	Launched	Peak Altitudes (miles)	Maximum Speed (feet per second)	No. of Static Firings	No. of Flight Attempts	
1	May 3, 1949	50	3,450	2	3	Premature cutoff due to steam leaks in turbine.
2	September 6, 1949	32	2,675	1	2	Same as Viking 1.
3	February 9, 1950	50	3,440	2	1	Rocket cut off by radio when westward drift became excessive.
4	May 11, 1950	105	5,160	1	2	Shipboard firing. Peak altitude almost the maximum possible.
5	November 21, 1950	108	5,150	2	1	Long burning time due to reduced thrust.
6	December 11, 1950	40	4,030	1	1	Night firing. Fins failed. Rocket executed violent maneuvers.
7	August 7, 1951	136	5,865	1	1	Highest flight of original airframe design.
8	June 6, 1952	4	——	1	0	Rocket broke loose on static firing and destroyed itself.
9	December 15, 1952	135	5,795	3	2	First successful flight of larger airframe.
10	May 7, 1954	136	5,720	2	2	Motor exploded on first flight attempt. Rocket was rebuilt and flown.
11	May 24, 1954	158	6,300	1	1	Highest Viking flight to date.
12	February 4, 1955	144	5,900	2	1	Successful flight. Last minute holds almost caused cancellation.

TABLE II. FLIGHT TIMES, MOTOR THRUSTS, WEIGHTS (VIKINGS 1 THROUGH 12)

Viking No.	Launched	Burning Time (seconds)	Time to Peak (seconds)	Time to Impact (seconds)	Motor Thrust (pounds)	Gross Weight (pounds)	Payload (pounds)	Oxygen (pounds)	Alcohol (pounds)	Peroxide (pounds)
1	May 3, 1949	54.5	164	291	20,450	9,650	464	3,250	3,445	160
2	September 6, 1949	49.5	133	394	20,465	9,935	412	3,580	3,420	265
3	February 9, 1950	59.6	169	420	20,450	11,050	528	4,160	3,870	280
4	May 11, 1950	74	242	435	20,450	11,450	959	4,170	3,830	280
5	November 21, 1950	79	248	450	18,800	11,390	675	4,380	3,780	270
6	December 11, 1950	70	145	292	20,800	10,890	373	4,250	3,850	260
7	August 7, 1951	72	266	530*	21,080	10,730	394	4,210	3,740	255
8	June 6, 1952	61	50*	100*	21,400	12,810	—	5,800	3,240	330
9	December 15, 1952	99	287	540*	21,170	14,625	765	6,075	5,280	390
10	May 7, 1954	100	290	595	20,990	14,750	830	5,900	5,530	385
11	May 24, 1954	103	309	557	21,400	15,005	825	5,810	5,790	385
12	February 4, 1955	102	299	540	20,500	14,815	887	5,600	5,815	390

*Estimated

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FROM WALLOPS ISLAND TO PROJECT MERCURY

1945-1958: A MEMOIR[+]

Robert R. Gilruth (USA)[++]

INTRODUCTION

The group that created the Mercury concept came largely from the old National Advisory Committee for Aeronautics (NACA). The Missile Range at Wallops Island, operated by the NACA, was a major factor in the development of this group, hence the title of this paper, "From Wallops Island to Mercury." The concept of the Mercury capsule, indeed, the whole plan for putting man in space was remarkable in its elegant simplicity. Yet is was a daring and unconventional approach at the time of its inception, and a subject of considerable controversy. However, the project demonstrated principles that were so sound they were also applied in the design and operation of the Gemini and Apollo flight programs.

The period covered by this memoir extends from the founding of Wallops Island as a missile range in May 1945, through the establishment of the Mercury Project in 1958. It is a period that saw great change, not only in science and technology, but also in world history. With the advent of the space age, the old NACA faded away to become NASA (National Aeronautics and Space Administration). Many of the people who had worked on and developed Wallops Island research projects helped form the nucleus of the Space Task Group, the group that would manage the Mercury Project. They were joined by others from NASA and by specialists from the Army, Navy, and Air Force, and also from industry.

Much of the early work at Wallops Island and in NACA was done in support of the ballistic missile program in the United States. Had it not been for the ballistic missile development effort we would not have had the knowledge of reentry bodies, guidance systems, or other factors such as the launch rockets themselves that were to make possible manned flight in space in such a brief span of time after the space age arrived. The first

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[+] Presented at the Sixth History Symposium of the International Academy of Astronautics, Vienna, Austria, October 1972.

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American astronaut orbited the Earth a little over three years after NASA was created. Even so, the Soviets were the first to put man in space. Yuri Gagarin in Vostok 1 would fly nearly a year earlier than John Glenn in Friendship 7.

This memoir is based primarily on my own reflections and recollections of this period. I have also discussed special events at length with Paul Purser, who was my close associate throughout the entire period covered by this memoir, and had recent discussions with Al Eggers and with Max Faget, both of whom played key roles in high-speed aerodynamics and also in satellite design.

In preparing this paper, I have used extensively the History of Wallops Station,[1] written by my former colleague, Joseph A. Shortal, who succeeded me as head of the Pilotless Aircraft Research Division (PARD). That organization operated Wallops Island and conducted the research program carried out there. His excellent history gives the complete story of the development of facilities and techniques used at Wallops Island. In particular, I have used many photographs and figures that Shortal collected from the old files at Langley Field.

THE BIRTH OF THE WALLOPS ISLAND MISSILE RANGE

The Wallops Island Missile Range was created during the final phases of World War II, at a time when the type of warfare and the weapons being used were undergoing rapid change. The flight speeds of aircraft were approaching the speed of sound and problems of aerodynamic shock waves and compressibility effects were assuming very serious proportions. In other areas, the guided missile had emerged as a major weapon of high potential. The V-1 buzz-bomb was harrassing the London area, and the V-2 rocket had just made its appearance. There were other forms of guided missiles in Germany that were playing an increasingly important part in the air war. In the Pacific, the Japanese Kamikaze suicide missile, of dread capability with its human guidance system aboard, was very difficult for us to cope with and caused much havoc with our naval forces. The Allies had put primary effort into the development of superior aircraft, and the appearance of so many guided missiles was a technical surprise. The new jet engines and rockets opened a wide spectrum of weapon possibilities, and the newly created atom bomb made the consequences of all these things seem far more serious.

At this time, the National Advisory Committee for Aeronautics was the principal aeronautical research establishment in the United States. It was responsible for aerodynamic research and for other fields of scientific endeavor dedicated to the improvement of aircraft. However, the high speeds of diving aircraft and supersonic missiles had overwhelmed NACA, for there were no wind tunnels in its laboratories that were capable of model testing for aircraft or missiles at airspeeds that approached close to and exceeded the speed of sound. In fact, wind tunnels of that time had a blind spot that extended through a whole speed region around the speed of sound (Mach number 1) due to a choking of flow in the test section of the tunnel. The NACA had concentrated its work on subsonic aerodynamics and its efforts in the transonic range had been confined to studies of shock wave-boundary-layer interactions during the early onset of compressible flows. The fast pace of events of the war made it essential to proceed much more rapidly with aerodynamic research. New techniques and solutions to problems were urgently required. We also needed to raise our sights and to gear up for a larger part of what obviously was to be a new national effort in weapon research and development.

It was at this time that new facilities for research on the problems of guided missiles and high-speed flight were proposed by the leaders of the NACA with the backing of high officers of the Army and Navy. A detailed account of the wartime work done by the NACA on guided missiles and of events leading to the establishment of the Wallops Island Station is given in Reference 1. Military officials proposed to Congress that a free-flight guided missile range[+] be built and operated by the NACA. Geographic surveys were made up and down the East Coast of the United States for a location where such free-flight experiments could take place, yet be convenient to the NACA research center at Langley Field. At that time, a tracking range of about 50 miles southward along the coast was believed to be adequate for research purposes. Wallops Island, a strip of land on the Atlantic Ocean, met this requirement and had unlimited range directly out to sea. It was selected in April 1945.

In May 1945, after Congress had approved this new facility and had appropriated money for buildings and equipment,[++] I was relieved of my duties in the Flight Research Division at Langley Field and put in charge of this new work. There was no fanfare about it. Mr. Crowley, the Acting Engineer-in-Charge of the Langley Center, just called me in and told me that I was to manage this new activity and that he had confidence that I was the man to do it. Prior to this time, I had worked in flight research after arriving at Langley Field in 1937. My work involved an extremely close working arrangement with test pilots in establishing handling quality requirements for airplanes.[2] In some of this work, I did a great deal of flying as an engineering observer, and I had developed a keen appreciation for the pilot's side of the man-machine relationships. I learned a great deal from Chief NACA Test Pilot, Melvin N. Gough, in particular. He was very interested in airplane handling qualities and took great pains to educate me in both pilot and airplane characteristics. This background was to be very important later in our work on Project Mercury, and throughout Apollo, in decisions regarding the roles and authority of man in the space capsule.

[+] They also proposed that NACA build a supersonic wind tunnel which later was authorized for the Ames Laboratory in California.

[++] Funds in the amount of $4,536,000 were appropriated for the construction of facilities and the first year's operation, Reference 1.

I was also deeply involved at that time in transonic aerodynamic research using (1) free-falling, instrumented bodies dropped from high altitude, and (2) the wing-flow technique,[+] which I had invented to help with the problems of aerodynamics near and through the speed of sound. Some of this work is described in my paper on the development of the wing-flow technique,[3] that covers behavior of wings and controls at sonic speeds and illustrates the importance of thin wing sections for aircraft designed to fly men through the speed of sound. This work was available in time to be considered in the wing selection for the X-1 research airplane. It provided the impetus for selection of the very thin wing (by standards of those days) which first broke the sound barrier in 1947. My supervisor, Floyd Thompson, Chief of Research of the Langley Laboratory, made the decision based on data we had obtained with the wing-flow technique.

While I hated to leave the Flight Research Division, I recognized the opportunity of developing a whole new free-flight research facility, and soon I became completely engrossed in the new job of creating and operating the Wallops Island Missile Range. Wallops Island is a barrier island off the East Coast of the United States. It is a low-lying spit of land separated from the mainland by a salt marsh, and populated by many sea and game birds. Some of the most delicious clams and oysters in the world are to be found in this vicinity. The island itself was populated by miniature wild horses, voracious horse flies, and some of the hungriest mosquitoes I have ever seen. The north end of the island was wooded, while the part we were to utilize in our early operations consisted of sand and sea grass with a few salt water bushes. In the next few months, work went ahead in the design and construction of facilities on the island. All materials of construction had to be barged from the mainland to the island. Boats alone were used for many years to transport men and equipment. In 1959, after the creation of NASA, a causeway was built across the marshes which permitted rapid access by automobile and truck, and greatly increased the work capacity of Wallops Island. Of course, by this time, I was no longer in charge of Wallops Island. Robert L. Krieger had become Director of the Station.

I learned many things during those early days of building and operating the Wallops Island range. I was a young man of 31 with little experience except in research matters. I found a whole new world of budgets, land acquisition, hiring, recruiting, and operating with other agencies and companies, as well as with the community around Wallops Island. We had planned all along to retain Langley Field as our research base, using Wallops Island principally as a test site where research missiles and models of various

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[+] The wing-flow technique gave aerodynamic results over the speed range from Mach number of 0.85 to 1.2, the very range which the wind tunnels could not cover. It made use of the fact that the air above the wing of an airplane went quite smoothly and uniformly through the speed of sound. This region of airflow on a P-51 airplane was used to test models of wings and controls. This method and a variation of it in wind tunnels was used extensively for several years.

kinds could be flown without danger to local communities. The models were prepared at Langley and carried over to Wallops Island by air or boat where they were fitted with boosters, internal power, and sustainer rockets. The radars, tracking equipment, and telemeter receivers were located at Wallops Island, but the research work on new telemeters and the instrumentation would be done at Langley Field. This plan was best suited to those times. It gave us the highly sophisticated technical laboratory with all facilities at Langley Field for the research staff, and yet we had the remote spot where the research flight tests could be carried out. This plan continued in effect for many years, and it was only after the advent of the space age and the creation of NASA in 1958 that Wallops Island became the independent NASA Station which it is today.

EARLY GOALS AND ACHIEVEMENTS

Those who first conceived the need for Wallops Island believed that flight trials of guided missiles for the Army and the Navy would be conducted there. However, it was soon evident that the service organizations were going to want their own guided missile ranges. The Navy was to develop such facilities at Point Mugu and at Inyokern, and the Army already was in business at White Sands, New Mexico. The Banana River Test Range at Cape Canaveral later became the guided missile range of the newly formed United States Air Force.

I determined to begin operations at Wallops Island as early as possible so that we would be able to incorporate our operating experience into the design of the permanent facilities. Our operations during the first year were reminiscent of advanced base activities in the war in the Pacific. We used surplus equipment and material, such as landing craft from the United States Navy, pierced steel plank for roads and landing ramps, jeeps, and other kinds of gear that had been developed during the war. Our first launching of a research vehicle occurred in August 1945, only a few months after the start of Wallops. A model of the air-to-air missile, Tiamat,[+] was flown out over the sea in a flight simulating inputs from a guidance system through an autopilot. A view of the Tiamat model on the launch pad during launch preparations is shown in Figure 1. I personally attended all launchings in those days, in charge of launch operations, and made decisions during launch preparations and the final countdown. We operated out of a blockhouse made of sandbags. Our firing leads and cabling to the launch sequencing equipment were strung out over the sand. It took a great deal of enthusiasm and individual initiative to work in the heat, amidst the mosquitoes, horse flies, and with sand in our food. Actually, the Tiamat test Was quite successful for what it was intended. However, we could see that the missile was

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[+] Tiamat was officially the Army Air Forces MX-570 project, an air-to-air missile. According to Babylonian mythology, Tiamat was a sea monster; Reference 1.

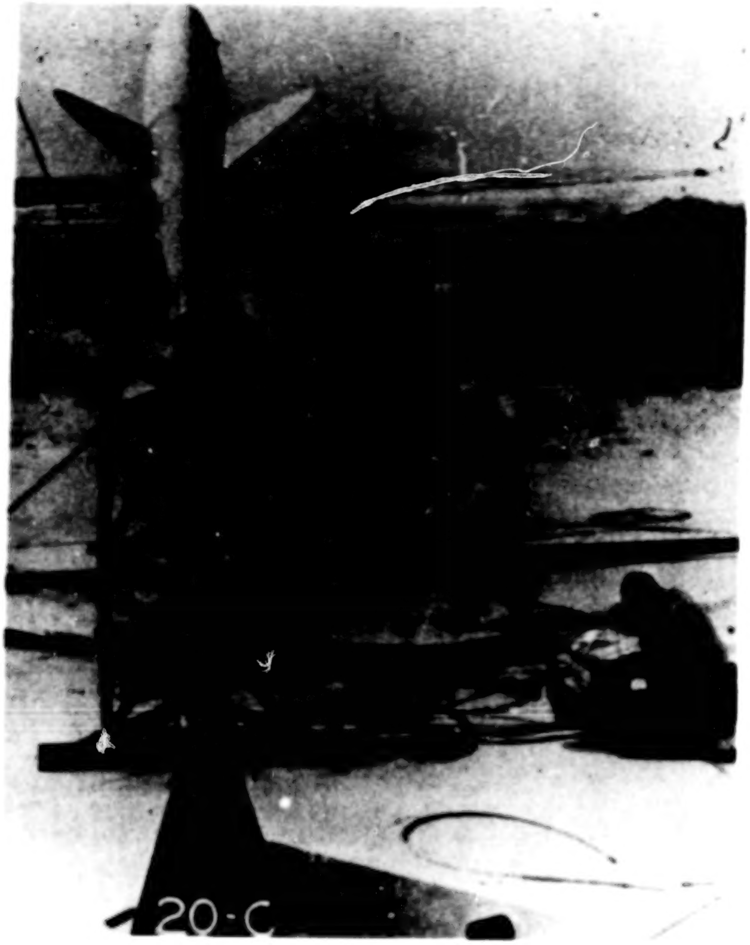

Fig. 1
View of Tiamat Test Vehicle and Booster on
Launching Pad, Wallops Island, July 1945

a very complex and expensive device to construct, instrument, and launch, even if we had had adequate and permanent facilities.

It soon became apparent to me that the original plan, which called for launching a large number of these complex missiles of Army or Navy designs, would take all of our resources and leave little remaining for work on the principal problems of the time, which had to do with transonic and supersonic flight characteristics of new and advanced aerodynamic concepts. From almost the very first, therefore, we began changing the purpose of the Wallops Island range. Although originally conceived as a missile testing site, it became a missile research range. Here we developed otherwise unobtainable basic information on the aerodynamic and structural behavior of wings, bodies, and controls, and on other key items in missile and aircraft design from rocket-powered models in free-flight at supersonic and transonic speeds. In the process we acquired a nucleus of very skillful and creative people in this research with rocket-propelled models. Key people in this very early group and their specialties are listed in Table I.

TABLE I

KEY PEOPLE[+] AND THEIR SPECIALTIES AT WALLOPS STATION

Paul E. Purser	- General Aerodynamics & Structures
Maxime A. Faget	- Aerodynamics & Propulsion
Paul R. Hill	- Aerodynamics & Propulsion
Joseph G. Thibodaux	- Propulsion & Materials
William J. O'Sullivan	- Propulsion & Aero Sciences
David G. Stone	- Stability & Control
Caldwell C. Johnson	- Design of Flight Models
Edmund C. Buckley	- Overall instrumentation system: radar, telemetry, sequencing, checkout
Morton J. Stoller	
Charles A. Taylor	
George B. Graves	- Telemetering
Paul F. Fuhrmeister	- Doppler Radar
Gerald M. Truszynski	- Tracking Radar
Robert A. Gardiner	- Guidance & Stability Equipment
Robert L. Krieger	- Tracking Radar - Operations Management

We devised many specialized techniques for this research over the next several years. We had to seek low cost methods of model construction, instrumentation, and propulsion, to keep within our small budget, and yet cover significant numbers of configurations of interest to designers of new aircraft and missiles. Most of our research models were relatively small, and we used solid rockets almost exclusively for propulsion because

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[+] The organization relationships are described in detail in Reference 1. Ray Hooker was my original deputy during the days of building the Range. Buckley became my deputy in January 1948 because of the importance of electronic interface with the research programs. Later Shortal replaced Buckley as my deputy and became head of PARD in 1952 when I took other duties.

they were adequate to give us the speeds that we needed, and were simple and relatively inexpensive. In fact, standard aircraft rockets were available to us free of charge from the Navy. The solid rockets were fitted inside the fuselages of our research models, which were often constructed of wood with metal inserts to have the required aerodynamic shape and the necessary strength and stiffness. An early drag research model in its special launcher is shown in Figure 2. Systematic studies were made of wing and body drag using simple models of this type.

NACA
LMAL 45928

Fig. 2
An Early Drag Research Model in its Special
Launcher at Wallops Island, October 1945

A Doppler radar, TPS-5, obtained from the Army (Figure 3), was used to track the models in coasting flight so that we could measure the drag of configuration without having to put instruments into the model itself. This was possible because the radar was sufficiently precise in its measurement of velocity that one could accurately determine the deceleration of differentiating the velocity-time curve. This measure of deceleration, with the known mass of the model, gave us the drag forces versus Mach number. Radiosondes were used for atmospheric data. A typical velocity-time curve for an early drag test is

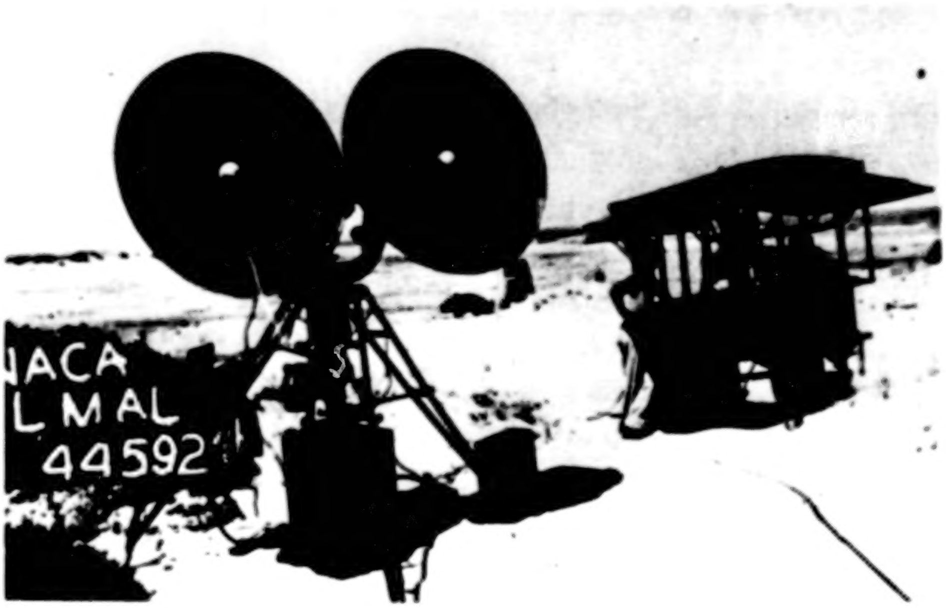

Fig. 3
Early Doppler Radar (AN/TPS-5) Used at Wallops Island
in Initial Aerodynamic Drag Studies

shown in Figure 4. After burnout, the slope of the curve is a measure of the drag. Figure 5 shows the results of systematic study of the effects of sweep-back and aspect ratio on the drag at supersonic and transonic speeds. As time went on, the radars were improved and greater accuracy and range were obtained. Systematic tests were made with larger models and over a greater speed range. Figure 6 shows typical results from systematic tests of various fuselage shapes.

Drag measurements of this type became our most important product during those early years. I recall, however, the first real trial of the technique occurred during a visit of George W. Lewis, Director of Aeronautical Research in Washington, and Jerome C. Hunsaker, Chairman of NACA, both men of great prestige and authority. As luck would have it, the model we launched to show how the technique worked lost its wings due to a structural failure. Hunsaker immediately said, "So the technique is no good; it doesn't work." I recall replying, "No, Dr. Hunsaker, we simply have to learn how to make the wings stronger." This we were able to do, and for the next ten years or so Wallops was to measure the drag characteristics of literally hundreds of new airplane and missile designs, as well as make systematic studies of wing and body drag and of other items.

Data on the effectiveness of various types of controls were provided by another very simple technique that utilized a polarized signal from a small radio transmitter in the model to measure the rate of roll as the model coasted through the speed range from supersonic to subsonic speeds. The effectiveness of deflected ailerons on winged models

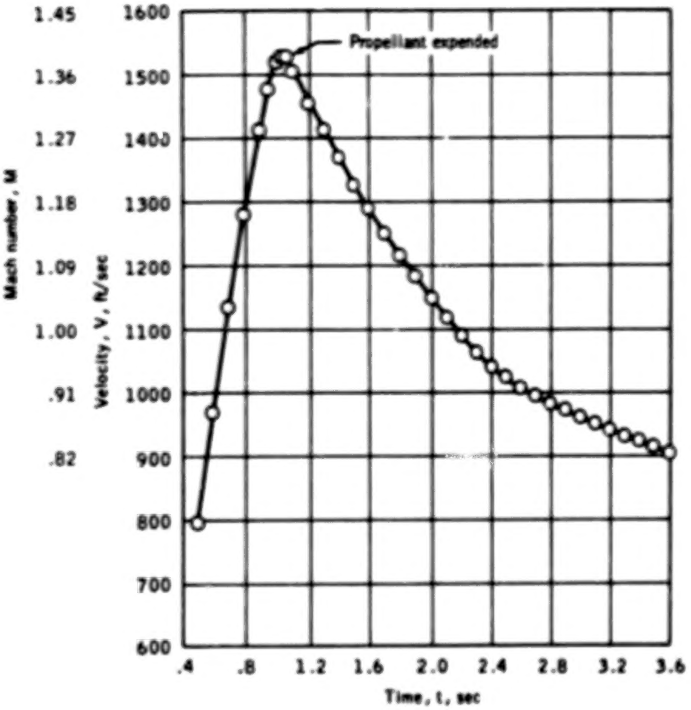

Fig. 4
A Typical Velocity-Time Curve for a Drag Research Model

in producing rate of roll was a direct measure of the aileron power of the design at the various speeds encountered. In this way, we measured the properties of various kinds of ailerons and spoilers on swept and unswept and tapered wings of various aspect ratios. These results found immediate use in the design of missiles and high-speed aircraft. A typical test result is shown in Figure 7, where reversed aileron effectiveness is cured by using a blunt trailing edge on the aileron.

During this early phase of Wallops Island work, the systematic data on the drag and control effectiveness was also used as a yardstick for comparison with other techniques, such as the wing-flow technique in-flight and the transonic bump technique in wind tunnels. Conventional wind tunnels were still unable, because of the choking problems, to provide useful data for aircraft and missile configurations at speeds close to the speed of sound.

As our abilities increased, we were able to provide free-flight data on the wing flutter characteristics and pressure distributions, as well as on dynamic behavior of complex wing-body arrangements. Using our new free-flight techniques, we provided information for the design of the X-1, X-2, and X-3 research airplanes. Model tests of new fighter aircraft were made at Wallops Island while they were still on the drawing board.

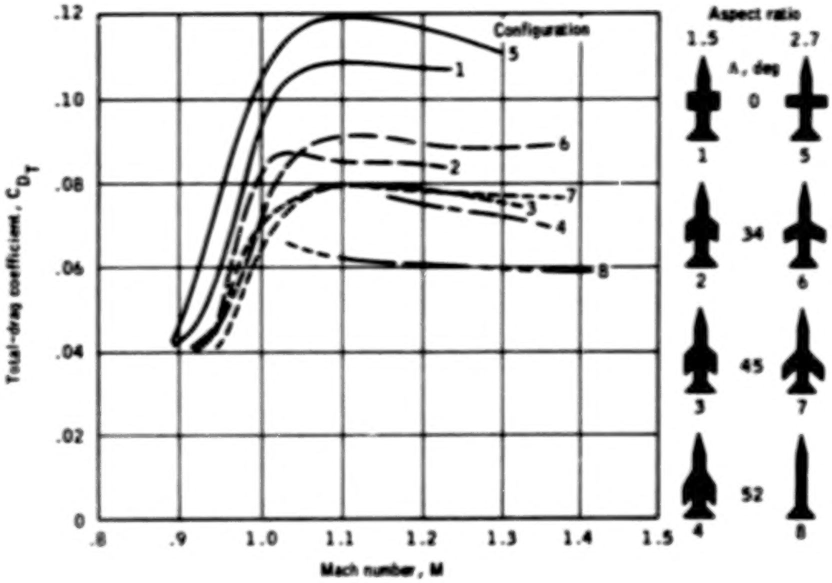

Fig. 5
Effects of Wing Plan Form on Drag From
Early Tests at Wallops Island

The investigations usually concerned drag and stability through the speed of sound. The North American F-100 fighter was tested in this manner, as were many others. In the missile field, models of the Rascal, the Snark, the Sparrow, and the Navaho were flown at Wallops Island, to name only a few.

During these early years of guided missiles and supersonic flight, the ram jet was considered to be an engine of great promise, and a number of ram jet test vehicles were flown at Wallops Island. One such program involved the NACA Lewis Laboratory at Cleveland, Ohio, which had prime responsibility for ram jet engine research. Their program used free-flight vehicles air-launched over the Wallops Island range. The concept was to mount the test vehicle underneath a carrying airplane at the Cleveland airport, and then take off and fly to the vicinity of Wallops Island. Once over the Island and acquired by tracking radars, the airplane would proceed on a drop run with its test vehicle underneath. The ram jet engine would then be activated and the test vehicle launched on a prescribed trajectory. The radars would track its flight while the telemeter receivers at Wallops Island would record the instrument records of the engine operation. This technique proved to be very successful, and many such drop tests were made in the years of 1949 and 1950. Key people in this Lewis Laboratory work were Scott Simpkinson and John Disher, both of whom joined the Mercury Space Task Group in later years.

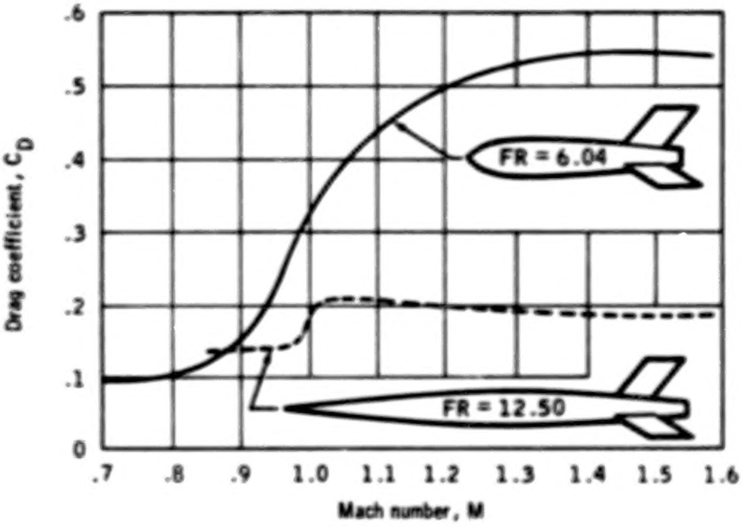

Fig. 6
Effect of Fuselage Shape on Drag From Wallops Island Tests

A second ram jet program involved a Langley group under Paul Hill and Max Faget that specialized in ground-launched test vehicles using ethylene fuel and burners of short combustion length. The configuration consisted of a long central body containing the fuel tank with two ram jet engines mounted as nacelles on either side of the horizontal tail. The first ram jet of this type was flown in March of 1950, achieving a Mach number of 3.02, a new world record. A second flight a few months later achieved a Mach number of 3.2 and the test vehicle coasted on up to a height of over 130,000 feet. Hugh Dryden was greatly impressed at the performance of these small ram jets and took great pains to inform the military services of the success achieved at Wallops Island. However, both the Air Force and Navy were indifferent to this work, and showed little interest in following up on the Langley designs.

After these initial successes, ram jet work gradually lost favor to the increasing importance of the ballistic missiles in the national programs. The Langley work was phased out and the Lewis flight efforts turned to research on reentry bodies. It is of interest that a young engineer, named George M. Low, was working with Scott Simpkinson on reentry bodies air-launched at Wallops Island only a few years before they both joined the Mercury effort for flying man in orbit. The research vehicles flown by the Lewis Lab for measuring reentry body characteristics were able to achieve very large Reynolds numbers at high Mach numbers because an air-launch allowed the test vehicle to penetrate downward into the atmosphere achieving high Mach number in the dense lower atmosphere, whereas the test

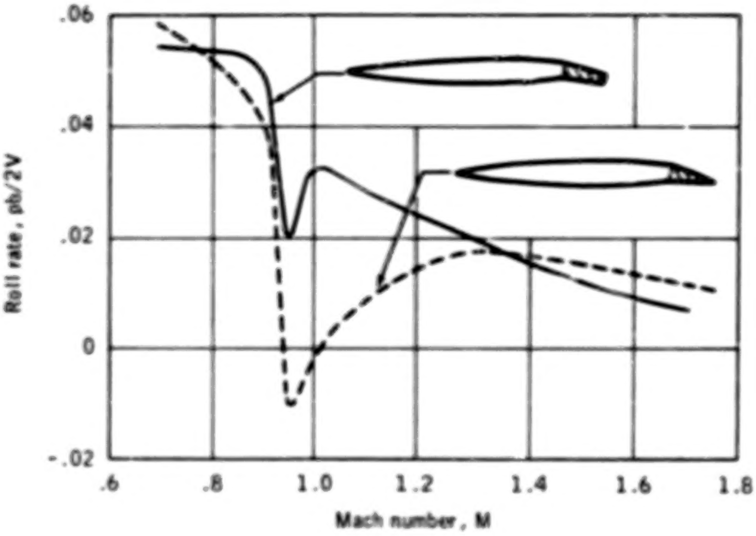

Fig. 7
Effect of Aileron Shape on Rate of Roll at Transonic
Speeds From Early Tests at Wallops Island

vehicles ground-launched from Wallops Island achieved high Mach numbers in the upper atmosphere where the Reynolds numbers were lower. Thus, the Lewis work made a good counterpart for the work done at Langley in contributing to the knowledge of aerodynamic heating on reentry bodies for ballistic missiles.

I cannot leave free-flight techniques without describing the stability and control work using "pulsed controls." In this technique, the elevators, or pitch controls of an aircraft or missile configuration, were moved abruptly up and down in a square wave pattern by a motor drive, and the response of the model in pitch was measured using accelerometers and an angle of attack meter. From these data it was possible to obtain not only the static stability, $\underline{dC_m}$ but also the lift curve slope, $\underline{dC_L}$ the neutral point, and the drag due to the lift $\frac{dC_D}{dC_L}$.

A variation of this technique employing "pulse rockets" was also used. Pulse rockets were simply small rockets specially designed to give pitch or yaw disturbances to the model during flight for the same purpose, i.e., stability measurements. A key instrument in all this work was the angle of attack meter. We developed our own device in NACA that consisted of a small delta-wing probe mounted in the undisturbed airflow forward of the nose of the fuselage.

But it was always a problem to obtain enough monetary support to do all the work that needed to be done. In free-flight testing, it was especially difficult to convince the Congressmen and other high officials that work was really worthwhile when the models had to be destroyed in the tests. People would always exclaim: "What a shame to let such a fine model destroy itself in the sea." Recovery efforts would have been much more expensive than the models at that time, and, in spite of the loss of the model with each flight, our costs were highly competitive with the supersonic wind tunnels which had very great initial cost and far less flexibility.

However, American industry, our principal customer, was impressed with the value of our results and they were not bashful in asking for more. A group called the "High Speed Subcommittee," which was led by Eugene Root of the Douglas Company, together with other members of the aircraft and missile industry, supported us in a solid manner. They recommended in a unanimous vote that the Wallops Island work be expanded by a factor of at least three. This recommendation was passed on through their parent Committee to the NACA itself. The NACA budget for the Wallops Island work was increased by a factor of three and this action greatly helped in expanding our work.

During this time of intensive work on new research techniques, I was appointed a member of a group in Washington called the "Planning Consultants to the Committee on Guided Missiles." Karl T. Compton, President of MIT, was Chairman of the parent Committee, the Committee of Guided Missiles, which operated in the Pentagon. It was the job of the Planning Consultants to review the nation's guided missile programs and to make whatever recommendations we felt would make for a stronger and more effective national effort. It was a great opportunity for me as a young engineer, whose specialty was aerodynamics and structures, to work so closely with these people from industry, university, and government, who were skilled in all the disciplines and in management as well.

While I served on this Board (1946-1947) our work covered the early period of the national guided missile program. This was the time when the Lark, the Hermes, the Bumblebee, the Navaho, and the Nike were among the major missile projects in the United States. The atom bomb was still so secret that even people working as Planning Consultants for the Committee on Guided Missiles were not privy to the information on the sizes and weights of these weapons. We made several recommendations to the Guided Missile Committee. The most important one, in my opinion, was that the ballistic missile had great promise as a weapon and that the technical problems in guidance and reentry could be solved. Of course, it was up to the Guided Missile Committee as to how our recommendations would be taken and it was several years before the national program to develop the ballistic missile was instituted.

In looking back over the years following World War II, the data and techniques developed at Wallops Island had an important and quite profound effect on U.S. Aircraft and missile designs. In later years, the Wallops Island group would gain competence and experience in dealing with the problems over almost the entire spectrum of high-speed flight. This background was to be a great help in moving into the space program when the opportunity came.

WALLOPS ISLAND: TRAINING GROUND FOR SPACE

I have been asked many times: "Why was Wallops Island such a good training ground for the space age?" Several factors were important. First, in a flight test project, the engineer had to consider the whole spectrum of problems. He learned about all the phases of his project even though he might only be seeking a drag curve. The model had to be designed and constructed with a power plant, a launcher, telemeter and instruments, radar tracking, operational crews, and so on. This broad kind of responsibility tends to attract and develop exceptional people. I found this to be true also in flight research with manned aircraft. One had to take a broad view; the detail thinkers tended to gravitate to the wind tunnel or other kinds of specialty work.

The fact that the research models were expendable and therefore had to achieve a very high reliability was also an important factor. For example, the pyrotechnics had to work, the second stage rockets had to ignite, the electronic systems had to survive the vibrations of the launch and the accelerations of the flight. We had to be concerned with structural integrity because the loads imposed on these models were high indeed. We had to be concerned with wing flutter and divergence, and we had to deal with problems of aerodynamic heating. Pressure was on the operational people to acquire the models with radar or the flight would be a failure, and we were operating over the sea, which gave us an intimate knowledge of the recovery environment with which we were to work later in recovery of manned space capsules.

We were using as everyday tools items that would be vital to space projects. This activity thus became a great training ground for the young people coming up. We learned the necessity of keeping our designs extremely simple and attacking a problem in steps. During the first two or three years, I insisted that we use no more than 4-to-6 channels of telemetry in a given model, and that we build our programs to minimize the effect of a loss by not involving too much effort in any one flight. Wherever possible we used the backup systems that were to become important in spacecraft design. However, the extreme weight sensitivity of these early rocket models made redundancy difficult to achieve except, of course, for very small, light items such as the electrical squibs used for igniting rockets and activating other devices.

NEW HORIZONS

In the fall of 1947, the X-1 research airplane, piloted by Major Charles Yeager, became the first manned airplane to break the sound barrier. Thanks to the results from the Wallops Island rocket models and earlier transonic research, there were practically no surprises. It was a great milestone of flight. Later that same year, John Stack and his associates at Langley Field built the first truly transonic wind tunnel. It was neither open nor closed, but had a slotted throat which permitted the flow to go quite smoothly through the speed of sound in the test section without the choking phenomena of the past. This type of wind tunnel was to be further refined and applied to wind tunnels all over the world.

Our work at Wallops Island began changing. There was less incentive for transonic testing with the improved wind tunnel capability, and our interests were increasing in higher Mach number problems. In 1952 Bob Woods, the Chief of Airplane Design for the Bell Aircraft Company, proposed a new research airplane, the X-15, for flying man to the very edge of space. I remember well his proposal, and equally well the people even in those times who said, "Why fly man? Why not fly an instrument?" My reply was: "Man will fly in space someday, so why not start doing it as soon as possible." This was the first time I heard the theme I would hear so often in later years reflecting the conflict between manned and unmanned flight. The X-15 became a successful research airplane, but the blunt space capsules in both the United States and Russia became the first true space vehicles.

In the guided missile field, heat transfer was becoming very important. Heat transfer research lent itself well to rocket model techniques at Wallops Island. Very accurate and useful measurements were made in free-flight using the skin of the missile as a calorimeter. This work started with research on slender bodies of revolution and in later years proceeded to concentrate on reentry bodies having blunt faces. Figure 8 shows a research missile designed for boundary-layer and heat transfer work at high Reynolds numbers at supersonic speeds. The model was designated the RM-10 and used for correlating flight and wind tunnel results to show the effects of Reynolds number and tunnel turbulence at various Mach numbers.

My own career was changing along with the changing research picture. I became an Assistant Director of the Langley Center, and my responsibilities broadened to include the Structures and Aerodynamic Loads Divisions in addition to the free-flight testing at Wallops Island. During the next few years we worked very hard to develop structures and techniques for high-speed flight and high temperatures. The work at Wallops Island went to higher and higher speeds using multistage solid rockets. In order to reach the high Mach numbers required, powerful new solid rockets were acquired and staging techniques reached a high degree of sophistication. In the years just prior to Sputnik, five-stage

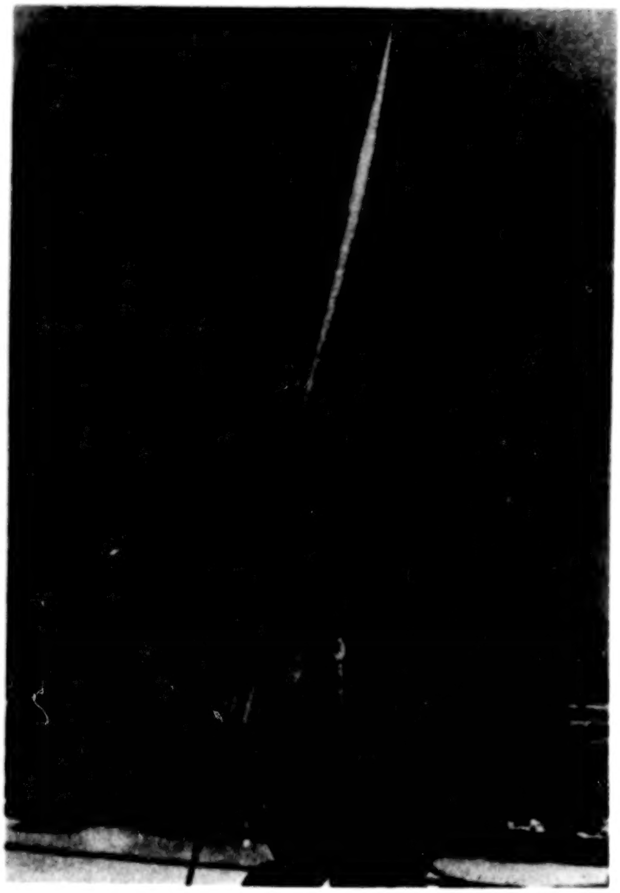

Fig. 8
Research Missile RM-10 Designed for Boundary-Layer and Heat Transfe
Research at High Reynolds Number and Supersonic Speeds

rockets were used to achieve airspeeds greater than Mach 15.0. A five-stage rocket designed to measure heat transfer on a blunt body is shown on the launch pad in Figure 9. The know-how acquired in designing multi-stage solid rockets was applied in later years in the development of the multistage rocket called the "Scout." The Scout was to launch satellites into Earth orbit from Wallops Island as well as from other locations. It is still frequently used.

At Langley Field, meantime, we were also creating new kinds of ground facilities such as arc jets, pebble-bed heaters (using alumina, zirconia, and thoria), and combustion jets of various sorts. Paul Purser worked with me on special assignment to create many of these new high-temperature facilities. Most of these were pilot models of ideas that were to be pursued further in the future, but they were also very productive of data on heat transfer and ablative materials for ballistic missile designs at that time. Figures 10 and 11 show the design of a zirconia pebble-bed heated jet which is still in service.

Fig. 9
Five-Stage Heat Transfer Test Vehicle Launched
December 1956, Wallops Island

Supersonic air jets with stagnation temperatures of 4000° were used for material testing. A larger facility of this type was later built at the Ames Research Center in California.

EVOLUTION OF THE MERCURY CONCEPT

The launch of Sputnik 1 on October 4, 1957, had a great effect on the thinking of all of us in the NACA. I can recall watching the sunlight reflecting off of the Sputnik 1 carrier rocket as it passed over my home on the Chesapeake Bay in Virginia. It put a new sense of value and urgency on the things we had been doing. When one month later the dog, Laika, was placed in orbit in Sputnik 2, I was sure that the Soviets were planning for man-in-space.

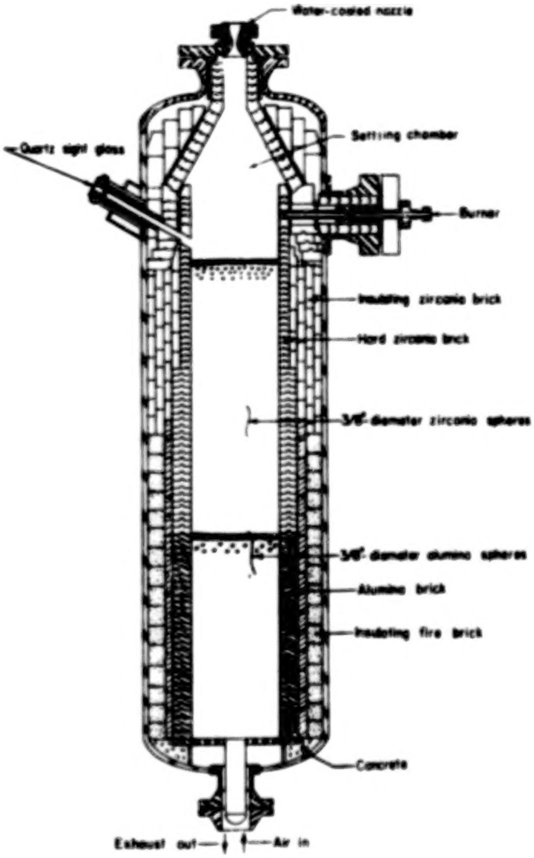
Water-cooled nozzle
Settling chamber
Quartz sight glass
Burner
Insulating zirconia brick
Hard zirconia brick
3/8"-diameter zirconia spheres
3/8"-diameter alumina spheres
Alumina brick
Insulating fire brick
Concrete
Exhaust out
Air in

Fig. 10
Diagram of Ceramic-Heated Tunnel at Langley Field, 1957

At that time, many of us started thinking intensively about manned satellites of one kind or another. It seemed to me that the United States would surely compete with the Soviet Union in space research. Prior to this, many of our research people had been studying manned hypersonic gliders. In fact, there was a high-priority research project sponsored by the Air Force for studying just such a vehicle. This work later became the basis for the Dyna-Soar Project that continued until the end of 1963 before it was cancelled. Another hypersonic glider, the X-15 research airplane already mentioned, was under development. It was designed for a maximum speed of about a Mach number of 7. According to histories of this time period,[4,5] work was being done on manned satellites even before Sputnik. I do not recall having contact with this work except for that of Harvey Allen of the Ames Research Center. He was the first, to my recollection, to propose a blunt body

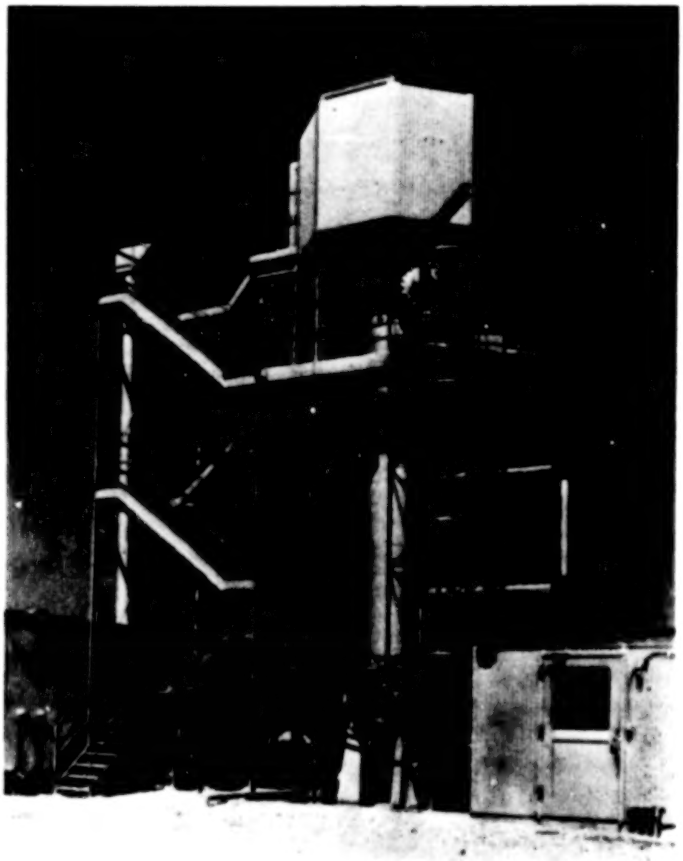

Fig. 11
Photograph of Ceramic-Heated Tunnel at Langley Field, 1957

for manned satellites. He suggested a sphere to enclose the man and said, "You just throw it," meaning, of course, launching it into orbit with a rocket.

In the fall of 1957 we held a meeting to discuss "Round 3" at the Ames Research Center in California. "Round 3" was a term for the next step for research aircraft beyond the X-15. As it happened, the Soviets launched Sputnik the week before the meeting. The impact of the Soviet achievement on the meeting was the realization that orbital flight was a legitimate national goal. I could not attend the meeting, and Purser and Faget represented the Langley and Wallops group. They came away disenchanted with hypersonic gliders and convinced that a blunt body-type of manned capsule should be the next step in manned flight. Al Eggers of Ames had proposed a compact-type of glider, now termed a "lifting body," shaped like "half-a-baked potato." This was a clever design and one which would have inherent advantages in lower reentry g-forces.

In March 1958, at another conference held at the Ames Research Center, Max Faget presented a paper+ called "Preliminary Studies of Manned Satellites - Wingless Configuration: Non-Lifting." This was a very significant paper. It put forward most of the key items that we would use in conducting the Mercury Project. It showed that a simple, nonlifting satellite vehicle of proper design could follow a ballistic path in reentering the atmosphere without experiencing heating rates or accelerations that would be dangerous to man. It showed further that retrorockets of modest performance were adequate to bring the capsule down from orbital speed and altitude to a reentry into the atmosphere. It also described the use of parachutes for final descent and small attitude jets for controlling the capsule in orbit, during retrofire, and reentry. This paper concluded: "As far as reentry and recovery are concerned, the state of the art is sufficiently advanced so that it is possible to proceed confidently with a manned satellite project based upon the ballistic reentry type of vehicle."

Because of its great simplicity, the nonlifting, ballistic-type of vehicle was the front runner of all proposed manned satellites, in my judgement. But there were many variations of this and other concepts under study by both government and industry groups. The choice involved considerations of weight, launch vehicle, reentry body design, and, to be honest, gut feelings. Some people felt that man-in-space was only a stunt. The ballistic approach, in particular, was under fire since it was such a radical departure from the airplane. It was called by its opponents "the man in the can," and the pilot was termed only a "medical specimen." Others thought it just too undignified a way to fly. Even Hugh Dryden, who at that time was Director of NACA, labeled the ballistic capsule proposal of the Army's "Project Adam" the same as "shooting a lady out of a cannon." When we proposed a similar ballistic phase in the Mercury Project a year or so later, he approved it since it was by then a buildup phase to orbital flight in the proof-testing of the spacecraft.

Various design concepts under study by U.S. industry at that time are shown in Figure 12. These configurations were compared and discussed at a conference held at Weight Field in January 1958. Weights of satellites varied from 1,000 to 18,000 pounds, and a wide variety of launch vehicle combinations were proposed. Even the X-15 was in the running. Its backers thought they could "doctor it up" somehow to get it through the reentry heat phase and thereby make it an orbital vehicle. Arthur Kantrowitz of AVCO had a very interesting concept which would deploy a very large metal parachute in orbit to cause enough drag or braking action to cause reentry. During reentry, the metal parachute would actually become white hot from the aerodynamic heating. Nevertheless, the majority

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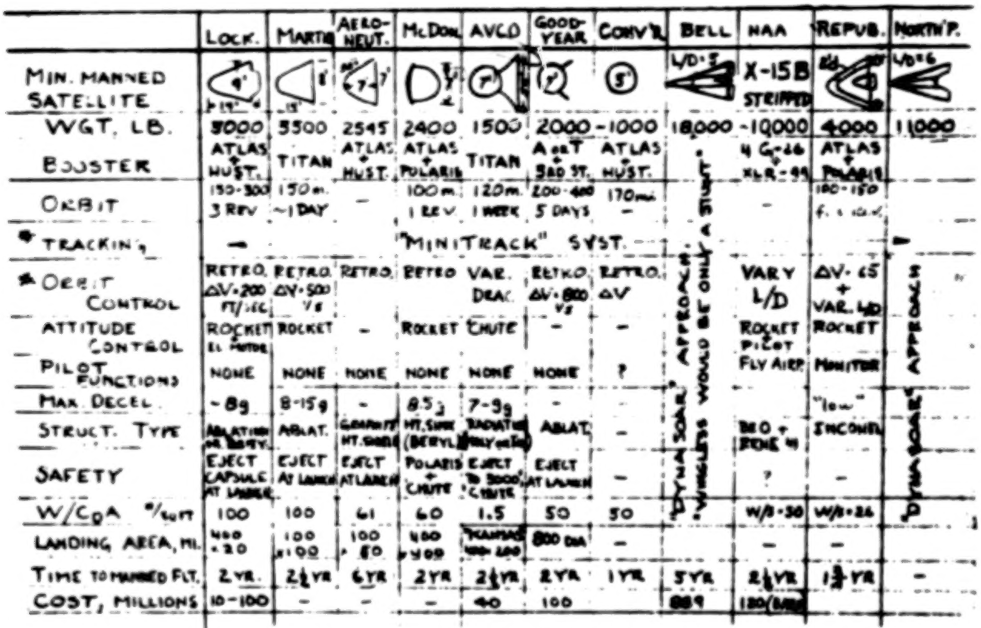
+Benjamine J. Garland and James J. Buglia collaborated with Faget in preparing this paper.



	Lock.	Martin	Aero-Neut.	McDon	AVCO	Good-Year	Conv'r	Bell	NAA	Repub.	North'P.
Min. manned satellite	9' 19'	8' 13'	7' 7'		7'	7'	5'	L/D=5	X-15B stripped	L/D	L/D=6
Wgt. lb.	3000	5500	2545	2400	1500	2000	-1000	18,000	-10,000	4000	11,000
Booster	Atlas + Hust.	Titan	Atlas + Hust.	Atlas + Polaris	Titan	A or T + 3rd st.	Atlas + Hust.	"Dyna Soar" approach. Wingless would be only a stunt	4 G-26 + XLR-99	Atlas + Polaris	
Orbit	150-300 3 Rev	150 m. ~1 Day	–	100 m. 1 Rev	120 m. 1 week	200-400 5 days	170 mi –		–	100-150 [illegible]	
* Tracking	—			"Minitrack" syst.							
* Orbit control	Retro. ΔV=200 ft/sec	Retro. ΔV=500 V_s	Retro.	Retro	Var. drag	Retro. ΔV=800 V_s	Retro. ΔV		Vary L/D	ΔV=65 + Var. L/D	"Dyna Soar" approach
Attitude control	Rocket El. motor	Rocket	–	Rocket	Chute	–	–		Rocket	Rocket	
Pilot functions	None	None	None	None	None	None	?		Pilot fly airp.	Monitor	
Max. decel.	~8g	8-15g	–	8.5g	7-9g	–	–			"low"	
Struct. type	Ablation or Beryl.	Ablat.	Graphite ht. shield	Ht. sink (Beryl.)	Radiation (Moly or Ta)	Ablat.	–		BeO + René 41	Inconel	
Safety	Eject capsule at launch	Eject at launch	Eject at launch	Polaris + chute	Eject to 3000' chute	Eject at launch	–		?	–	
W/C_DA lb/sq ft	100	100	61	60	1.5	50	50		W/S=30	W/S=26	
Landing area, mi.	400 × 20	100 × 100	100 × 80	400 × 400	"Kansas" 100-200	800 dia	–		–	–	
Time to manned flt.	2 yr.	2½ yr	6 yr	2 yr	2½ yr	2 yr	1 yr	5 yr	2½ yr	1½ yr	–
Cost, millions	10-100	–	–	–	40	100		889	120(...)		

* All non-winged designs use zero-C_L reentry with no flight path control. Winged designs have complex ground controlled systems monitored by pilot.

Fig. 12
Manned Satellites Under Study by U.S. Industry, January 1958

of the aerospace companies felt that some sort of ballistic shape should be used. Cost estimates varied from 40 million dollars to about 900 million.[+]

One concept of considerable interest, not shown in Figure 12, was proposed by Charles Mathews of Langley. It was the forerunner of the space shuttle designs of today. He proposed a circular-winged craft that would reenter at a very high angle of attack. In this mode of flight the heating rate would be greatly reduced and confined largely to the lower surface of the wing. Following the reentry heating phase, it would pitch over and fly to a landing like a conventional airplane. All these and other concepts were interesting and no doubt could have been made to work in due time. However, the most advanced ballistic missile at that time, the Atlas, could be expected to lift only about 2,000[++]

[+] It is of interest that the actual cost of the Mercury Project was about 400 million dollars. This cost covered everything, including the construction of the world-wide tracking range and the steaming time of the U.S. Navy for recovery purposes.

[++] The lift capability of the Atlas grew to more than 2,500 pounds by the time Mercury was ready to fly.

pounds into orbit. It seemed obvious to our group, therefore, that only the most simple ballistic capsule could be used if manned space flight were to be accomplished in the next few years.

During this period, I spent more and more time in Washington working as an assistant to Hugh Dryden, the Director of the NACA. There was a great deal of interest in space flight, and plans were going ahead at various levels in Government for the creating of a national space agency. Testimony was being given to the Congress by eminent people in science and technology on what they thought should be done in order to help make the United States competitive with the Soviet Union in this new space age.

THE HOT SUMMER OF 1958

In the early summer of 1958, a number of us from Langley Field and other laboratories of NACA went to work in Washington full-time to help Dryden and other members of his staff put together a plan and a budget for the new space agency that seemed certain to be created by the Congress that year. About 20 of us made our headquarters in one large room on the sixth floor of the old NACA building. There were about 10 telephones in the room, and we worked together and with others in Washington and around the country to create a plan and a budget. By this time, Abe Silverstein of the Lewis Research Center had been transferred to Washington, and Dryden placed him in charge of planning the entire space flight program.

Abe Silverstein and Hugh Dryden assigned to me to manage the man-in-space program during that hot summer of 1958, several months before NASA was created. I put together a plan that I hoped would be acceptable, not only to the people in NACA, but to the Advanced Research Projects Agency (ARPA), and, of course, to the President's Scientific Advisors. In order to do this, I collected a select group of people from Langley Field and from the Lewis Laboratory to form a sort of task force. Members of this group included Max Faget, Paul Purser, Chuck Mathews, and Charley Zimmerman of the Langley Lab, Andre Meyer, Scott Simpkinson, and Merritt Preston of the Lewis Lab, and many others on an "as needed" basis. George Low and Warren North of Lewis were brought in toward the end of the summer to help with the final plan, as was Charles Donlan of Langley.

During those humid summer days we came up with all of the basic principles of Project Mercury. The capsule would have a blunt face and a conically-shaped afterbody. It would be pressurized with a breathing atmosphere for the astronaut. The first real design to be put on paper was the work of Caldwell C. Johnson of the Langley and Wallops Island design groups. This original design is shown in Figure 13. Working closely with Max Faget and others at Langley, Caldwell Johnson continued to create successful spacecraft designs over the years. He helped create the designs for Apollo soon after doing those for Mercury. The escape tower was conceived by Max Faget, Andre Meyer, and some of their

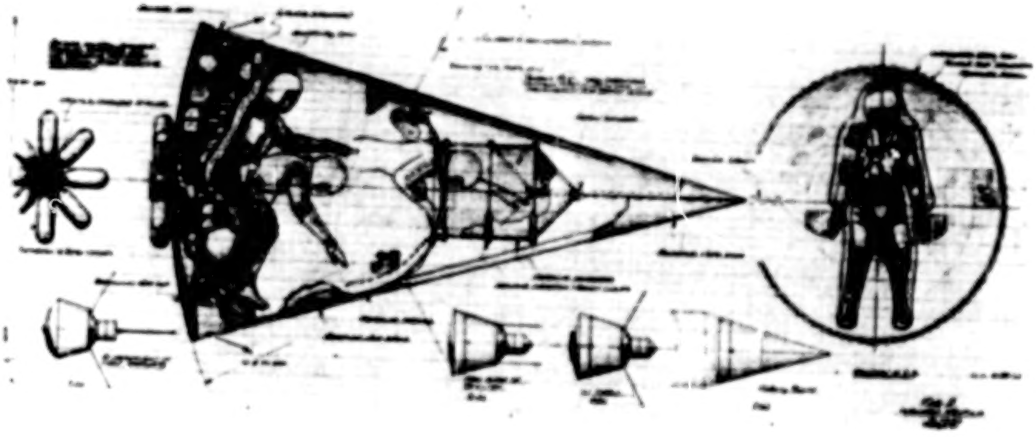


Fig. 13
Early Version of Mercury Capsule, Summer 1958

associates that same summer. The contour couch, another of Max Faget's ideas, that protected the astronaut against the high g-forces during launch and reentry, was the last key development in our plan that made Mercury possible. It was tested at the Johnsville centrifuge also that summer and proved effective for g-loads of over 20-g.

The launch vehicles proposed were the Atlas for the orbital phase of flight, with a Redstone used for early test flights of the capsule with man aboard. The Little Joe launching rocket, which was a new solid rocket cluster design proposed by Purser and Faget, was planned for use at Wallops Island as a means of testing the capsule configuration to gain experience with this type of flight vehicle before proceeding to the more expensive and difficult phases in the latter part of the program at Cape Canaveral.

The program did not yet have a name, but shortly after "go-a-head," Silverstein suggested we call it Project Mercury. This proved to be an excellent name and one we became very fond of. The basic principles on which we built the Mercury all emphasized simplicity. We wanted to use the simplest and most reliable approach--one with a minimum of new developments and using a progressive buildup of tests. It was implicit with this approach that we use the drag-type reentry vehicle, an existing ICBM booster, a retro-rocket to initiate descent from orbit, a parachute system for final approach and landing, and an escape system to permit the capsule away from a malfunctioning launch rocket. That was the way we went forward.

During July 1958, I made the presentation of this concept to the Scientific Advisor to the President, James Killian, and to the members of the President's Scientific Advisory Board. There were a number of members present who listened carefully to the presentation, among them: Edwin Land, George Kistiakowsky, and Killian himself. Herbert York, head of the ARPA Technical Staff, was also present. However, some of these gentlemen were not at all enthusiastic about our plan to put man into space. In fact, I was concerned at that time that they would not recommend that the program go forward at all. At one time in the hearings, Kistiakowsky remarked with great displeasure that our plan "would provide the most expensive funeral a man has ever had." However, when we left the hearings, Dryden was very pleased. He thought we had done well and he believed that the program would go forward.

We also appeared before Congress in presenting this program. The hearings held before the Select Committee on Aeronautics and Space Exploration on August 1, 1958, were the first disclosure to the public of how we planned to put man in space. I am impressed in going back over these hearings how very closely we followed what we said we would do. We said we would use the Atlas rocket, a special capsule with a blunt heat shield, and parachutes for landing at sea. All these things worked out very much as we had proposed. I was surprised at the public interest in our testimony. Crowds of people jammed into the small hearing room to look and listen during the presentations.

GO AHEAD - SPACE TASK GROUP

On July 29, 1958, The National Space Act was signed by President Eisenhower. The National Aeronautics and Space Administration was created and came into actual operation on October 1, 1958. T. Keith Glennan was appointed by the President to be our first Administrator. Hugh Dryden, the Director of the old NACA, was named the Deputy Administrator. NASA had been in business but a few days when we presented to Glennan and Roy Johnson, head of the Advanced Research Projects Agency of the Department of Defense, our plan for putting man in space. The presentations were the work of the Joint NASA-ARPA Panel for Manned Space Flight.+ Within two hours we had approval and a "go-ahead." Glennan advised me to get on with the project, go back to Langley Field, and put together a group to manage it.

The group, however, was not to report to the Center at Langley, but directly to Abe Silverstein in Washington Headquarters, who had now officially been made head of space projects in the new NASA. The Mercury presentation was made at the original NASA headquarters in the old Dolley Madison House in Washington, which had been fitted out for executive offices and conference rooms. Among those with me at the presentation were Donlan, Faget, and Mathews from Langley, Low and North of Lewis, Sam Batdorf of ARPA and, of course, Silverstein and Dryden. Paul Purser was not present as he was down at Redstone

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+Members of the Committee included: Robert Gilruth (Chairman), NASA, S. B. Batdorf, ARPA, A. J. Eggers, NASA (September 24), Max Faget, NASA, George Low, NASA, Warren North, NASA, Walter Williams, NASA (September 24-30) and Robertson Youngquist, ARPA (September 24).

Arsenal already starting negotiations for the Redstone boosters for the program. George Low was to have gone to Langley Field with me to help work on the project; however, Silverstein found out within a few weeks that he needed Low in Washington. Therefore, Low remained there as assistant to Silverstein in charge of the Manned Satellite Program, at the Washington level, until Mercury was finished and Apollo was well underway. He then joined us at the new Manned Spacecraft Center in Houston.

We returned to Langley immediately after the Glennan "go-ahead." We realized that we had been given a job of tremendous difficulty and responsibility. I had no staff and only verbal orders to return to Langley Field and "get on with the job." Floyd Thompson, my boss prior to this new assignment, was of great help in getting the effort going. When I asked him how I could get men transferred from the research center at Langley Field to my new Space Task Group, he suggested that a simple memorandum to him (stating that I had been authorized by the Administrator to draft people from the Langley Center) would allow me to name those whom I wanted. This is exactly the way we went forward. The historic memorandum is reproduced in the Appendix. A good share of the leadership in the U.S. space program eventually came from this group.

In those first early weeks we prepared a specification for the Mercury capsule that went out to industry with a request for their proposals within the next two or three months. As a matter of fact, the entire time span from project "go-ahead" in October 1958, through the request for proposal, bidders briefings, source selection activity, and placing of the contract, all occurred before the middle of January, less than four months later. This kind of performance could only occur in a young organization that had not yet solidified all of its functions and prerogatives.

During this same period of time we established an arrangement with the Ballistic Missile Division of the Air Force for the procurement of the Atlas launch vehicles and for launch services. We also worked out a plan with General Medaris and Wernher von Braun of the Army Ballistic Missile Agency for the Redstone launch vehicles, and we started work in our own staff for a design and specification for the Little Joe rocket to be used in tests at Wallops Island. We gave to the Lewis Laboratory, now a NASA installation, the job of creating a full-scale Mercury model spacecraft for an unmanned flight at an early date to establish levels of heat transfer and stability in a full-scale free-flight test on an Atlas booster at Cape Canaveral. Scott Simpkinson of Lewis was the key man in this project. He and his group, working with others at Langley under Jack Kinzler, created a spacecraft called "Big Joe," which was the first major step in proving the capsule design. Simpkinson and his people did the lower part of the capsule, the instrumentation, control system, and the heat shield, while Kinzler's group did the upper heat shield and the parachute deck. The project started in December 1958 and the spacecraft flew successfully in September 1959!

All of our people worked holidays, evenings, and weekends. We even worked on New Years Day that year, but we did take off New Years Eve. Those were days of the most intensive and dedicated work by a group of people that I have ever experienced. None of us will forget it. We were making tests of escape rockets over on the beach at Wallops Island; testing parachutes in full-scale drops from helicopters, and measuring water impact loads on capsule configurations at Langley Field.

In our early organization, I was the Director of the Space Task Group with Charles Donlan the Associate Director. Paul Purser served as my Special Assistant, Max Faget headed the Flight Systems Division, Charles Mathews the Operations Division and Charles Zimmerman, who came from the Langley Stability Division, became head of a group on Reliability and Quality Assurance. Our new contracts were handled by Sherwood Butler. We obtained physicians and flight surgeons with aero-medical training on loan from both the Air Force and the Navy, as well as some psychologists who were to help us in writing the selection procedures for the astronauts. Dr. Randolph Lovelace of the Lovelace Clinic in Albuquerque, New Mexico, agreed to head an Advisory Committee on space medical problems. He was a great help to us in dealing with the medical community in the early days of space.

The events and accomplishments of the Mercury Project have been documented many times. I will not try to cover them all here again. But there are a few memories and anecdotes that are interesting and have never been told before. One such memory has to do with the term "astronaut." I remember very well using this term to describe the men to be selected as flight crew members with Dryden. The question came up as to whether we should call them "astronauts" or "cosmonauts." Dryden was of the opinion that "cosmonaut" would probably be more accurate because astro, of course, applies to the stars, and we really were beginning to probe only the nearby cosmos. However, the way it turned out, everyone we talked to seemed to prefer "astronaut," and this was the name that stuck. That is fortunate, I believe, because now when we say "astronaut," we know we mean Americans, and when we say "cosmonaut," we know we mean Russians.

Initially our specialists in crew selection proposed choosing space pilots from people who had dangerous professions, such as race car drivers, mountain climbers, scuba divers, as well as test pilots. Few people realized then the degree of skill, knowledge, and training an astronaut would need. When President Eisenhower decided that astronauts would be chosen from a military test pilot pool, we breathed a sigh of relief. This was one of the best decisions in the program. It made it quite simple and logical to delegate flight control and command functions to the pilot of the satellite.

We returned again to Wallops Island in the Mercury Program to launch Mercury-type capsules with the Little Joe rocket (Figure 14). We started first with a research program just to get more flight experience with the new space capsule configurations. We had to be sure there were no serious performance and operational problems that we had simply not

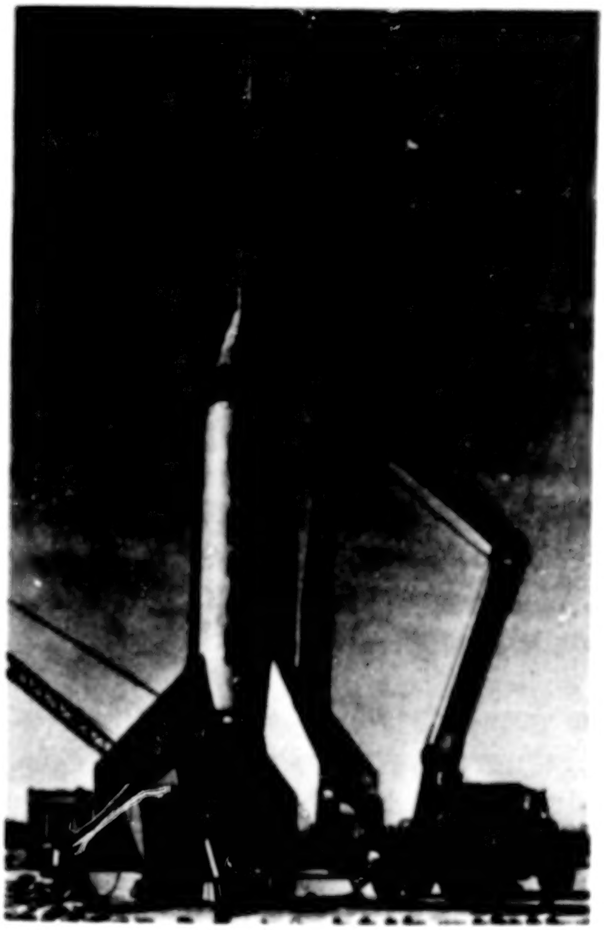

Fig. 14
Production Mercury Capsule and Little Joe Booster
Rocket in Background, Wallops Island 1961

thought of in such a new and radical type of flight vehicle. We also went back to Wallops in April 1961, before the first Vostok and Mercury manned flights, to test the McDonnell capsule at high dynamic pressures with the Little Joe rocket. The purpose of these tests was to ensure that the escape tower and the spacecraft would function properly in the abort mode at high dynamic pressures. These tests proved highly successful by uncovering some deficiencies in the limit switch system that sensed separation which, if they had not been corrected, might have caused unnecessary aborts in the flight tests that followed at Cape Canaveral.

Our Space Task Group was growing. We were hiring not only from Langley, but from Lewis and from other agencies of the Government, and from industry as well. Those days must have been particularly difficult for Langley and its Director, Floyd Thompson, because there was such a need for good people that we could not help but continue to

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recruit from the Langley Research Center. Floyd Thompson was very wise in the way he handled this. He told me one day, "Bob, I don't mind letting you have as many good people from Langley as you need, but from now on I am going to insist that for each man you want to take, you must also take one that I want you to take." So this is the way it happened. From that day forward, whenever we took a new man that we had recruited, we also took a man the Langley was eager to transfer.

During those early days we had technical reviews of the progress about every two months. We either journeyed to Washington with our charts and models to discuss with Administrator Glennan and his staff our progress and problems, or he would come to Langley Field with Hugh Dryden, Abe Silverstein, and others from Headquarters. These reviews were very good indeed because they set up milestones against which we could measure our progress. We were able to flag our problems for the Administrator, and we had plenty of them.

One of my problems in those days was getting authority from Washington to buildup an adequate staff. This was a project of an entirely different dimension than anything the NACA had ever done before, and even with my best efforts, I still had only a hundred or so people for a project which was growing in complexity and spending many millions of dollars a year. We had to cover many fronts, not only in the manufacturing area and the launch vehicle area, but also in the operations area as well. We had to develop a worldwide tracking net, and work out recovery operations with the U.S. Navy and Air Force. All in all, we had many, many tasks, and for a time I got very little sympathy from my superiors in Washington when I approached them with the need for more men. Fortunately, however, they were ultimately convinced, and we received the Civil Service billets we needed from the newly created Goddard Space Flight Center.

We selected astronauts in April 1959; we flew the Big Joe capsule, our first full-scale reentry test launched by an Atlas, in September 1959. In July we suffered a major setback when our first Atlas-Mercury production vehicle failed structurally under launch loads. These problems were not cleared up until the flight of Mercury-Atlas II in February 1961. In December 1960, we had our first successful flight test of a production capsule launched by the Redstone at Cape Canaveral. Things were beginning to fall into place, and we worked hard to accomplish the first manned flight in 1961.

THE END OF THE BEGINNING

Our first flight in 1961 launched the chimpanzee named "Ham." Ham was a friendly little fellow who received great play in the newspapers. His flight was highly successful from an aeromedical point of view. He was able to function perfectly during the period of weightlessness. He did all his chores and withstood not only the normal launch accelerations, but also a 20-g thrust of the escape tower which occurred because a Redstone timer was improperly set and had not deactivated the abort circuit at the time of main-stage burnout. This was to give the capsule an added push on its way to a new distance record for a Redstone and a landing point far away from the recovery ships.

After the flight of Ham and other tests on Redstone, Atlas, and Little Joe rockets, we were ready for manned flight. On May 5, 1961, Alan Shepard became our first man into space. Later that month, the decision was made to establish a goal of landing Americans on the Moon and returning them safely to Earth before the end of the decade. President Kennedy made that decision on the advice of Lyndon B. Johnson, the Vice President, James Webb, the new NASA Administrator, and with the unanimous support of the Congress of the United States. This truly marked the end of the beginning. John Glenn would fly successfully into orbit in February 1962, to be followed by Scott Carpenter, Wally Schirra, and Gordon Cooper--Cooper making the longest flight of the Mercury Program in May 1963.

The years from "Wallops Island to Mercury" saw the science of flight progress from the sonic barrier to manned satellites capable of orbiting the Earth. In looking back over those years, I think we were extremely fortunate to have developed the people and the capability for manned spacecraft design and operations. We were fortunate also to be there when needed, and even more fortunate to have been given the opportunity to participate in such an important and exciting part of world history.

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APPENDIX A

NASA - Langley
November 3, 1958

MEMORANDUM For Associate Director

Subject: Space Task Group

1. The Administrator of NASA has directed me to organize a space task group to implement a manned satellite project. This task group will be located at the Langley Research Center but, in accordance with the instructions of the Administrator, will report directly to NASA Headquarters. In order that this project proceed with the utmost speed, it is proposed to form this space task group around a nucleus of key Langley personnel, many of whom have already worked on this project.

2. It is requested, therefore, that initially the following 36 Langley personnel be transferred to the Space Task Group:

Anderson, Melvin S. (Structures)
Bland, William M., Jr. (PARD)
Bond, Aleck C. (PARD)
Boyer, William J. (IRD)
Chilton, Robert G. (FRD)
Donlan, Charles J. (OAD)
Faget, Maxime A. (PARD)
Fields, Edison M. (PARD)
Gilruth, Robert R. (OAD)
Hammack, Jerome B. (FRD)
Hatley, Shirley (Steno.)
Heberlig, Jack C. (PARD)
Hicks, Claiborne R., Jr. (PARD)
Kehlet, Alan B. (PARD)
Kolenkiewicz, Ronald (PARD)
Kraft, Christopher C., Jr. (FRD)
Lauten, William T., Jr. (DLD)
Lee, John B. (PARD)
Livesay, Norma L. (Files)
Love, Nancy (Steno.)
MacDougall, George F., Jr. (Stability)
Magin, Betsy F. (PARD)
Mathews, Charles W. (FRD)
Mayer, John P. (FRD)
Muhly, William C. (Planning)
Purser, Paul E. (PARD)

Patterson, Herbert G. (PARD)
Ricker, Harry H., Jr. (IRD)
Robert, Frank C. (PARD)
Rollins, Joseph (Files)
Sartor, Ronelda F. (Fiscal)
Stearn, Jacquelyn B. (Steno.)
Taylor, Paul D. (FSRD)
Watkins, Julia R. (PARD)
Watkins, Shirley (Files)
Zimmerman, Charles H. (Stability)

Robert R. Gilruth
Robert R. Gilruth
Project Manager

PEP.jbs

To personnel Officer for action.
This request is OK with the exceptions of Boyer. On Buckley's recommendation Substitute Kyle for Boyer.

F. L. Thompson,
Act. Assoc. Director 11-4-58

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Among other diciplines, Rocketry and Astronautics encompasses the physical and engineering sciences including fluid mechanics, thermodynamics, vibration theory, structural mechanics, and celestial mechanics. Papers presented in these two volumes range from those of empirical experimenters who used the time-honored "cut and try" methods to scientists wielding theoretical principals. The work traces the coupling of the physical and engineering sciences, industrial advances and state support that produced the awesome progress in rocketry and astronautics -- for the most part within living memory.

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